NATIONAL ACADEMY OF SCIENCES

OF THE UNITED STATES
OF AMERICA

BIOGRAPHICAL MEMOIRS

VOL. XXIV

CITY OF WASHINGTON
PUBLISHED BY THE NATIONAL ACADEMY OF SCIENCES
1947

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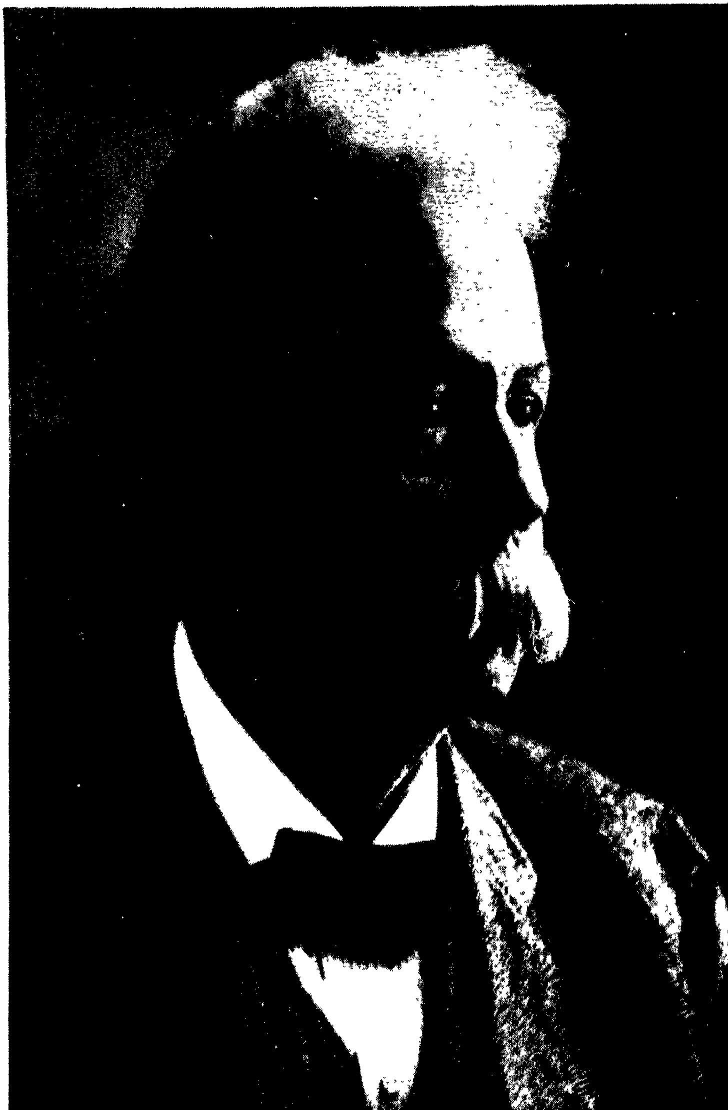

C. Hart Merriam

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XXIV—FIRST MEMOIR

BIOGRAPHICAL MEMOIR

OF

CLINTON HART MERRIAM

1855–1942

BY

WILFRED H. OSGOOD

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1944

CLINTON HART MERRIAM *

1855–1942

BY WILFRED H. OSGOOD

C. Hart Merriam, naturalist, zoologist, and leader for many years in a field of great activity, takes an honored place in the succession of such as Louis Agassiz, Asa Gray, and Spencer F. Baird. He was the central figure in a dynamic era connecting the pioneer period of exploration with the present time of experimentation and interpretation. To review his life is to recapitulate the events of this era and to indicate their far reaching effects not only in America but throughout the world.

Boyhood and Family History

He was born December 5, 1855 in New York City where his parents were then spending the winter. The family home, however, and the place of his boyhood days was in Locust Grove, Lewis County, New York. Here was the homestead with ample acres shadowed by the Adirondack Mountains and dating back to 1800. In such surroundings many a boy has a passion for the woods and fields even to the extent of beginning seriously to probe their secrets. Sometimes it may continue through life, but usually only as a pastime and a refuge from interests that are considered more practical. This boy, however, saw his career in it and from his early teens was engrossed with it. His parents encouraged rather than hindered it and the way opened up for him in a manner which probably even exceeded his boyhood dreams. That he had such dreams no one can doubt, for to the end of his days he continued to plan large undertakings.

He came of old and distinguished American stock, directly descended from Joseph Merriam who came with a brother from County Kent, England, and settled in 1638 in Concord, Massachusetts. Joseph's grandson moved from Massachusetts to

* For other biographical accounts of the same subject see Talbot and Talbot, Science, *95*, pp. 45-46, 1942; Fisher, Jour. Wash. Acad. Sci., *32*, pp. 318-320, 1942; Camp, Calif. Hist. Soc. Quart., *21*, No. 3, pp. 284-286, 1942; Osgood, Jour. Mamm., *24*, pp. 421-436, Nov. 1943.

favored few, and where he had made a valuable collection of birds, which we examined together with mutual interest."

In later life, Merriam frequently referred to this trip and there can be little doubt it was a powerful influence in shaping his career. It gave him a taste of the great West which he subsequently quartered so thoroughly and it connected him directly with the era of its geographic exploration which was just closing. The Pacific Railroad surveys were not very far back, the men who participated in them were still living, and the spirit of discovery was still rife. Baird, especially, as Secretary of the Smithsonian Institution, had shown the possibilities of promoting zoological knowledge through the cooperation of governmental agencies and it is not unlikely that Baird, directly or indirectly, planted the seed in Merriam's mind that later bore so much fruit.

Education and Early Influences

In 1872 and 1873 he prepared for college, first at the Pingry Military School in Elizabeth, New Jersey, and later at Williston Seminary of Easthampton, Massachusetts. In 1874 he went to Yale where he spent three years in the Sheffield Scientific School with his interest in natural history getting stimulation from such men in the faculty as A. Hyatt Verrill, Sidney I. Smith and Daniel Cady Eaton. Among his classmates was the biologist Edmund B. Wilson. He developed an interest in anatomy and with his roommate began dissections of the human body using cadavers surreptitiously obtained through a morgue in New York. This interest was stimulated by a Dr. Bacon of New Haven for whom he frequently acted as assistant in surgical cases. This led to the desire to make medicine and surgery his career, so he went from Yale to the College of Physicians and Surgeons of Columbia, receiving his M.D. in 1879, at the age of twenty-four.

While in New Haven, his interest in birds continued and before he left there or shortly after, in 1877, he published an account of "The Birds of Connecticut," bringing down to date a previous list published by J. H. Linsley in 1843. In the intro-

duction to this work he makes the significant statement that "distribution . . . is unquestionably governed (as shown by Professor Verrill) by the temperature *during the breeding season.*" Therefore, this thesis, which he later so abundantly elaborated, seems to have been derived from Verrill. In 1874, as indicated by a brief published paper "Notes from the South," he had an outing in Florida with his father and in 1875 he was temporarily employed at Woods Hole, Massachusetts, by the U. S. Fish Commission. Results of his observations of birds in the vicinity of Locust Grove were published in several short papers (1878-1881). On March 7, 1878, while a medical student in New York, he assisted in organizing the Linnaean Society of New York and was elected its first president. During these years he made many lifetime friendships and continued to expand his connections with young naturalists of the period.

At this time, ornithology was receiving much attention in various quarters and especially in Cambridge, Massachusetts, where a coterie of young men was actively engaged in forming private collections of bird skins. These included William Brewster, Henry Henshaw, Ruthven Deane, Henry Purdie, and Charles Batchelder. This group in 1871 organized the Nuttall Ornithological Club and Merriam was one of the early contributors to its "Bulletin," which began in 1876.

From 1879 to 1885 he followed the practice of medicine in Locust Grove, apparently with marked success, but the record shows that during this time he was also very active in studying the local fauna, in building up his collections, in correspondence and personal contact with others of similar taste, and in developing a growing interest in mammals as well as birds. In the spring of 1883 he went to Newfoundland and the Gulf of St. Lawrence as surgeon of the S. S. *Proteus,* engaged in a sealing expedition which gave him the opportunity to observe the great migration of seals and to collect many specimens. The series of skulls obtained, especially of the hooded seal, is still the finest in existence. On this trip he made the acquaintance of Napoleon A. Comeau of Godbout, Quebec, who thereafter sent him numerous specimens for many years.

In 1881 he published a "Preliminary List of the Birds of the Adirondacks" but, thereafter, although active in promoting ornithological work, his personal effort was devoted almost entirely to mammals. This effort began at home and the energy, patience and thoroughness he gave to it were evidenced by the appearance in 1884 of his "Mammals of the Adirondacks," a large comprehensive book almost exhaustive in character and setting a new standard for local studies. For its time, it was quite unusual, comparable in some respects to certain studies of birds, but surpassing anything of the kind relating to mammals. It gave little attention to description and classification, these at the time being taken for granted or regarded as closed subjects. It was rather a series of "life histories" giving every shred of information obtained through his own observations since boyhood and everything trustworthy he had been able to derive from others. It was a remarkable piece of work for one scarcely more than a beginner and it is still a valuable source of information not yet superseded. After issuance in parts in the Transactions of the Linnaean Society of New York, it appeared as a handsomely bound volume of small quarto size and dignified character. Possibly it was subsidized by his father whose interest was somewhat more than paternal. It is significant that it was an ambitious undertaking, planned on a comprehensive scale, and the forerunner of much that came later.

It is probable that the "Mammals of the Adirondacks" was only a part of a much larger plan, which even at this early date he had conceived. He had the works of DeKay on New York State and he had read Humboldt and Wallace and become greatly interested in the underlying problems of the geographic distribution of animals. He began to consider the idea of a general biological survey of New York State and went so far as to employ a clerk to search meteorological records and especially to compute mean temperatures for each month in the year. He obtained introductions to James Hall, C. D. Walcott, and others and interviewed them in Albany with the object of enlisting their aid in obtaining an appropriation for the proposed survey from the state legislature. Failing in this, he settled to the

practice of medicine and went on with his study of the mammals. Among various foreshadowings of his future which appeared when he was scarcely more than a youth, this effort to obtain state aid is perhaps one of the most significant.

American Ornithologists' Union

In 1883, such a great interest in ornithology had developed throughout the country and so many distinguished ornithologists were located outside New England that the Nuttall Club, although continuing as a local society, was subordinated by the formation of a larger, more truly national organization, The American Ornithologists' Union, modeled somewhat on the British Ornithologists' Union but inspired by the fellowship of a remarkable set of men. Its founders, who held their first meeting in New York, Sept. 26, 1883, formed a varied group, the like of which can never again be brought together. Their names, with few exceptions, are now on the honor roll of achievement in a subject that has numbered its followers by thousands with many fascinating ramifications extending to every state in the Union and beyond our borders to a great part of the world. Looking back at these men now, one finds it difficult to avoid the feeling that they measured to an average stature beyond that of any later period. Whether they were the products of their times or vice versa, at least it must be said that they and their opportunities were well met. The seniors in this group were such as Spencer F. Baird, George N. Lawrence, Charles E. Bendire, Elliot Coues, J. A. Allen, and Robert Ridgway. Merriam was one of the younger members and must have been greatly influenced by his associations. Nevertheless, he immediately made himself felt. He was elected secretary of the new organization and became chairman of its important Committee on Bird Migration, a subject on which preliminary work had been started by Prof. W. W. Cooke. The large-scale collection of factual data was almost a passion with him and this committee offered his first chance to exercise it in a broad way. He made elaborate plans (see the Auk, vol. 1, pp. 71-76, Jan. 1884), dividing the country into thirteen districts, each having a super-

intendent, and undertook correspondence with all of them and the collation of their data. It was a nationwide project which just suited him and which doubtless was suggestive of possibilities in other directions.

Growing Interest in Mammals

Meanwhile, notwithstanding the impetus given to ornithology by the organization of the A. O. U., Merriam's interest in mammals continued to grow and his private collection of them was enlarged to proportions which at that time were unprecedented. Private collections of birds were numerous, some of them even rivalling those of public institutions, but similar collections of mammals were practically non-existent. The readily obtainable mammals of any given locality were few in number and there was no general interest in them. Bird collectors and local taxidermists sometimes preserved them but in small numbers and mostly including only diurnal species, such as squirrels and rabbits which fell to their guns while afield with other objectives. It was from such sources that many of his specimens came and he carried on a large correspondence, promoting interest in mammals by purchasing specimens and, in some cases, by employing collectors or at least by placing standing orders. Among those with whom he was in touch, about 1883, was a farmer's boy of Elk River, Minnesota, Vernon Bailey by name. This boy's specimens included so many species thought to be difficult to obtain that Merriam gave him exceptional encouragement. It is related that he once asked Bailey for specimens of shrews, tiny mammals of nocturnal and secretive habits then supposed to be rare. Bailey replied, saying "How many do you want?" At that time specimens of shrews were derived mainly from something the cat brought in, something that fell in the well, or something found dead and decayed in the road, so Merriam then wrote Bailey "all you can get." Some time later, Bailey sent him no fewer than sixty shrews and it is not unlikely that then and there he envisioned the possibilities of a continental campaign of mammal collecting. Until then his formation of a mammal collection had been a dubious venture which probably would

have failed if undertaken ten years earlier. The time was exactly right for it not only because he had found a successful collector but because there had just been devised a small trap adapted for use in field and forest as well as on the pantry shelf. This trap, called the Cyclone, was an affair of tin and wire springs, only about two inches square when collapsed, cheap in cost, and easily portable in quantity. Also, high standards had just been reached in the preparation of mammal skins and skulls following the example set by the ornithologists who had carried the art of making study skins of birds to a high degree of perfection.

In the beginning, it is possible that Merriam's interest in mammals had been due partly to general interest in zoology and partly to a certain perversity or contrariness which followed him through life. Most of his contemporary associates were ornithologists only, with a large following of amateurs and a subject of established method and great popularity. It was not in his nature to follow the herd, however, and although birds were his earliest interest, never wholly relinquished, he probably began the special study of mammals before he fully realized its possibilities. Whether or not he was favored with something akin to the Midas touch, it is certain that he rose to every occasion with energy, determination, and a personal magnetism which carried him to success.

In the early eighties what was known of American mammals, to say nothing of the rest of the world, was pitifully little. Small and, from present standards, very scrappy collections were in the American Museum at New York and the National Museum at Washington. The specimens were poorly prepared, imperfect, and often lacking in data. One or two of a kind was the rule and the number of kinds was scarcely greater than had been known to Audubon and Bachman thirty years before. Mammalogists, as such, were practically unknown and the few studies that had been made were those of men who were primarily ornithologists, notably S. F. Baird, Elliot Coues, and J. A. Allen. The subject was not only neglected; it was thought to be unproductive and sufficiently canvassed. The larger mam-

mals were supposed to be of uniform character throughout the country and, with a few exceptions, the existence of the smaller ones was scarcely even suspected.

Into this situation then came the combination of Merriam, Bailey, and the Cyclone trap, and of these no one will ever deny that Merriam was the greatest. He immediately gave Bailey further commissions and as soon as he was able kept him in the field almost continuously. In 1884 he described his first new species, *Atophyrax bendirii,* a small shrew obtained for him in Oregon by the ornithologist Major Charles E. Bendire. His collection of mammals had then reached a total of some seven thousand specimens and for working purposes was probably superior to any public collection.

Founding and Growth of Biological Survey

At this time, however, although carrying on the medical practice in Locust Grove, he was occupied with his plans for a broad study of bird migration through the committee of the Ornithologists' Union of which he was chairman. This work had grown to such proportions it was evident the A. O. U. could not carry it without help and the enlistment of the aid of the Federal government was considered with the result that in the spring of 1885 Congress authorized the establishment of a section of ornithology in the Entomological Division of the Department of Agriculture. The A. O. U. was consulted in the choice of a man to take charge of the work and through Professor Baird, whose influence doubtless weighed heavily in the whole matter, Merriam was invited to take the position with the title of Ornithologist. At the time, Merriam was in Europe where he had gone partly for a respite from medical practice, which had become rather onerous, and partly to promote his interest in migration studies. In 1884 he had been appointed with Elliot Coues to represent America on an international committee for the world wide establishment of bird observation stations.

He had visited various naturalists, notably Blasius of Braunschweig and Graf von Berlepsch of Gertenberg, near Kassel,

Germany, but after receiving the offer he cabled his acceptance and returned in time to take up the duties of the new office July 1, 1885. Meanwhile, for Assistant Ornithologist, he had invited Dr. Albert K. Fisher with whom he had developed a warm friendship while they were fellow students at the College of Physicians and Surgeons in New York—a friendship which lasted for more than sixty years. With one clerk, Merriam and Fisher, neither of whom had yet seen his thirtieth birthday, then started what has since become a bureau with hundreds of employees and millions of dollars in appropriations. Within a year, they obtained independent rank as the Division of Economic Ornithology and Mammalogy, a title which in later years was sometimes jestingly and perhaps a little maliciously changed colloquially to "Economic Ornithology and *Extravagant* Mammalogy." Later, the simpler title Division of Ornithology and Mammalogy was used. In the beginning, growth was not very rapid, but in the late nineties there was considerable expansion and on March 3, 1905, the title was again changed to Bureau of Biological Survey. Very recently (1939) and long after Merriam's retirement, the work was transferred to the Department of the Interior where it was joined with the former Fish Commission and the two now go on as the Fish and Wildlife Service.

A critical period for the young organization was passed in its first year when it succeeded in divorcing itself from the entomologists. This was done against the opposition of Dr. C. V. Riley, then head of the Entomological Division. Merriam's father at this time was retired from Congress, but he still had many influential friends, among them, especially, Senator Warner Miller, then Chairman of the Senate Committee on Agriculture. Fisher also had a friend in Representative Stuhlracker and by combined efforts, perhaps with the assistance of Baird and G. Brown Goode, who were very friendly to Merriam, Congress was induced to establish the independent division with a total appropriation of $10,000 (raised from $5,000 received the first year) as of July 1, 1886.

After this, it was plain sailing, at least for a number of years. As the chief of a growing scientific bureau in Washington, Mer-

riam soon became a national figure and for twenty-five years of his activity in this post his career was crowded with interest and accomplishment. During the first few years, work was mainly in the completion of projects previously outlined and perhaps designed to justify their federal support, since their bearing on agriculture and public welfare was obvious. Early additions to the staff were Walter B. Barrows, W. W. Cooke, and F. E. L. Beal, all of them so-called "economic" ornithologists. Very shortly (1888 and 1889) two exhaustive "Bulletins" were published, "Bird Migration in the Mississippi Valley" by Cooke and "The English Sparrow in America" by Barrows, the first especially being replete with evidences of Merriam's careful editing and supervision. Somewhat later (1892) came Fisher's "Hawks and Owls of the United States in their relation to Agriculture," a thorough study, handsomely illustrated, which was received with much acclaim and which is now a classic. These three excellent bulletins were widely circulated and established public esteem for the work of Merriam's division, doubtless contributing to its continued support and growth.

Exploration and Research

Meanwhile, Merriam had other and longer range plans, especially for himself. Vernon Bailey had been employed as field agent and was ranging the West, sending in previously unknown species of mammals with every shipment, and the study and description of these received Merriam's enthusiastic attention. He obtained authority for inauguration of a technical series of publications by the Department of Agriculture under the title "North American Fauna" and in the first number an announcement stated that "The Division of Ornithology and Mammalogy is engaged in mapping the geographical distribution of birds and mammals, *in addition* to a study of their economic relations. The purpose of this work is to ascertain the boundaries of the natural fauna areas of North America." Thus at an early date (1889) his broad plan was clearly envisioned and his future career outlined.

In the first four numbers of the North American Fauna and

within two years (1889 and 1890) he described no fewer than seventy-one new species as well as several new genera of mammals. To zoologists throughout the country this was startling, unprecedented, and to some who doubted his standards it even seemed preposterous. It was soon evident, however, that he was not splitting hairs unduly, but was announcing real discoveries. Others began to adopt his methods not only in America but abroad and an era of discovery in mammalian classification was started which even now has not fully run its course and which has provided a basis for innumerable studies and interpretations ramifying in many directions.

As soon as practical, he arranged for a personal expedition to the West, going with Vernon Bailey to the San Francisco Mountain region of Arizona and studying the distribution, not only of mammals, but also of birds, reptiles, and plants. The results of this expedition, published in August 1890, outlined much that was later built upon to establish his reputation as an authority on the subject of the geographical distribution of animals and plants. There was much discussion of life zones, of the origin of faunas, the causes determining distribution, and besides maps of the special area there was a large colored map of North America showing the principal life areas as he conceived them. In subsequent years he wrote and published various papers on distribution, and revised his maps from time to time, but all his fundamental ideas appear in this early report.

Thenceforward he became passionately interested in pursuing his studies of mammals and in carrying the distributional work to the whole of North America. He gathered around him a circle of naturalists and collectors and posted them out from season to season in all directions. In 1891, he organized a larger expedition, the Death Valley Expedition, which explored the mountains and deserts of southeastern California and Nevada with very good results. This expedition was placed in charge of Dr. T. S. Palmer, since Merriam himself, after starting with the party, had been invited by President Harrison to act as a Bering Sea Commissioner to study the fur seals and spent the summer on the Pribilof Islands in Alaska. While work was

going on in various parts of the United States, he also sent parties to Canada and to Mexico where E. W. Nelson and E. A. Goldman worked continuously for a number of years. Eyebrows in some quarters lifted at this spending of Federal funds in foreign countries but he justified it as part of a continental job basic to everything else. Although really a man very sensitive to criticism, he usually acted without regard to it, following his bent in spite of it.

From the beginning he made large plans for the study of mammals from all points of view. Not only classification and distribution, but anatomy, ecology, nomenclature, bibliography, and specialized regional studies were among them. Added to these were botanical and general zoological subjects. At first it seemed he was personally ambitious to conduct a whole legion of projects himself and in fact he made preliminary studies in nearly all of them, frequently accumulating considerable data which would be laid away to take up something new. One of his early plans was the compilation of the vernacular names of animals in which he enlisted the cooperation of the publisher, L. S. Foster of New York and L. O. Pindar of Kentucky. A list of the technical names, especially of mammals, was also on his agenda. In both cases his accumulated data were eventually turned over to others and at least some have been published. One of his later enthusiasms was botanical, a proposed monographic study of the manzanitas *(Arctostaphylos)*, especially those of California and the western United States. For this he collected quantities of material, shelves and cabinets of his office for several years being crowded with carefully labeled bottles containing the dried fruits of these plants.

The result was that his subordinates who came to him with a bright idea usually found themselves anticipated and if rebuffed there was sometimes feeling. In such cases, those who knew him best were able to see that he was not acting from pure selfishness but because of a perfectionist complex which made him honestly feel that he not only had prior rights but that he could trust no one else to carry out the study in all the detail he had had in mind. The enormous collection of mammals that

accumulated under his direction was deposited in the National Museum but with the strict agreement that no one should have access to it except by his order. With minor exceptions, the first ten numbers of the North American Fauna were written by himself and it was inevitable that sly references to him as a czar or a dog in the manger should now and then be heard. In the late nineties, however, he evidently began to revise his all-embracing ambitions and thenceforward many projects were assigned to his staff or to others competent to carry them out. Once this was begun, he freely gave advice and assistance, spending many hours revising the manuscripts of younger men or carefully reviewing their material with them and discussing their conclusions. His standards were high and his methods were worked out with the utmost care. In effect, he founded a school and the output of "Merriam and Merriam's men" was generally admired and respected. From time to time young men of his staff were drawn to institutional positions in various parts of the country where his influence was continued. In certain centers, notably in California, his methods were applied to restricted areas with outstanding results and even in Europe, especially in the British Museum, he was something of an inspiration.

Political Difficulties

While he was so actively promoting the study of mammals and what was called life zone work, especially in the West, other lines of activity in the Biological Survey were not being wholly neglected, but they were left to subordinates. Food habits research went on effectively under such men as F. E. L. Beal, A. K. Fisher, S. D. Judd, and W. L. McAtee. Game preservation and conservation work began to be important in the late nineties and these were cared for largely by T. S. Palmer, who for many years was Merriam's principal administrative assistant. Although having some of the characteristics of a promoter and a reformer, Merriam was not very worldly and not very diplomatic. As a bureau chief it was his duty each year to go before congressional committees and defend his estimates for appro-

priations. This was very distasteful to him and whenever possible he sent one of his assistants. Such committees are notoriously overbearing and as Merriam was too independent to submit to grilling, he sometimes gave them rather sharp answers. Although his own career had a political foundation, he himself was not politically minded. His attitude toward certain types of short-sighted Congressmen was so transparent that their resentment was unnecessarily engendered and eventually (1908) the agricultural appropriation bill was reported out of committee with the section usually devoted to the Biological Survey entirely omitted. Rumors at once started to the effect that it was because of his disproportionate interest in research rather than directly practical work, but the real reason was probably involved in the personal antipathies of certain Congressmen, plus congressional opposition to President Roosevelt who was known to be friendly to the Biological Survey. Merriam was dumbfounded and at his wits end. By his own efforts it is doubtful if he could have extricated himself from the dilemma, which struck not only himself but his whole staff; others, however, had been forehanded in marshalling popular pressure against such an emergency and before the bill came to a vote the missing items were reinstated. Not long after this Merriam sent for his old friend Henry W. Henshaw, who was then in Hawaii, and brought him into the Biological Survey as Administrative Assistant with the rather obvious function of appeasing criticism.

Breadth of Interests

During his most active years, Merriam was interested in many things besides mammals and the Biological Survey. New and improved methods of doing things fascinated him and every forward looking movement in scientific circles had his active cooperation. A curious exception was his failure to appreciate the advantages of modern card systems for filing and classifying data. In his early work on migration he had devised systems of his own which were too cumbersome for the vast later accumulations of information and, although he must have seen

this, he continued to insist, with some truth to be sure, that card indexes were "an invention of the Devil to kill time."

He was a member and one time president of the Biological Society and of the Anthropological Society. He was also a member of the Philosophical Society and a founder of the National Geographic, serving on the Board of Directors of the latter for no less than 54 years. He was Chairman of the U. S. Board on Geographic Names for nine years (1917-1925) seldom missing a meeting. He was active in founding the Washington Academy of Sciences and was especially devoted to the launching of its publications. He was also one of those consulted in laying the original plans for the Carnegie Institution. When in 1919 the American Society of Mammalogists was founded he was elected its first president. In itself, this might not be significant, but to those familiar with the history of American mammalogy it marks the expression of a rarely equalled preeminence of one man in a given field of science. But for him a society of mammalogists might not have been possible and, although younger men were more active in promoting the organization, no other could have been thought of for the honor of presiding at its opening sessions. His position was unique and everyone recognized it.

On April 17, 1902, he was elected a member of the National Academy of Sciences and when its meetings were held in Washington several of its members were always billeted in his home. In 1899, he organized and directed the Harriman Alaska Expedition, a unique affair in which he took great delight. When Mr. Harriman, who had been advised that Merriam was the man to carry out his plan, called on him in his small office and announced his desire to take a vacation with his family and fifteen or twenty of the leading naturalists of the country, Merriam had never heard of him and at first thought he was being hoaxed. He was soon convinced, however, and agreed to select the galaxy which finally made up the party. On the return, he undertook the editing and publishing of the handsome series of twelve volumes reporting on the many-sided observations of the Alaskan cruise. This work he did at home in his spare time,

not only editing the manuscripts but giving meticulous care to every detail of typography, paper, format, and illustration.

Throughout his life he was given to enthusiasms which were not exactly hobbies, for they were usually constructive, but since he did everything with his might, they often led him away from his main course. Through his practice in supervising the publications of the Biological Survey he began to investigate the qualities of printing papers. Instead of accepting some authority, he undertook an exhaustive study of the entire problem, familiarizing himself with every detail and finally joining with H. W. Wiley, then chemist of the Department of Agriculture, in publishing an authoritative bulletin covering the subject. For several months, paper was constantly uppermost in his mind and for long after he seldom lost an opportunity to discourse on it. Surviving members of the old Biological Survey staff will recall the light-brown manuscript paper, thought to be easy on the eyes, which he prescribed for use by the entire personnel.

For many years he spent most of his summers in the West traveling with wagon or pack train, returning to Washington for the winter. When the automobile began to be generally used, it suddenly struck his fancy and he developed a strange enthusiasm for everything pertaining to motor cars. He made several transcontinental trips with his family at a time when roads and cars were far from perfect; he haunted salesrooms and repair shops and his house desk was filled with catalogues and circulars. He knew every make of car from its emblem to its horse power and he carefully weighed the relative merits of every detail. His own car he was convinced was the best and he was always ready with arguments to prove it. To his intimates, who knew he had neither training nor natural capacity for engineering and mechanics, this was little less than amusing, but they could also see in it an expression of his very unusual character. He cared little for politics, current affairs, or general literature, but for art, especially painting, he had considerable enthusiasm. He openly scorned orthodox religion, and society in the usual sense he regarded as pure waste of time. But wherever his interest was really roused his whole power went into

it with a tremendous urge to get at the facts and to take no one's word for them.

Interest in California

His field work in the West gradually concentrated on the great state of California not only because it offered many interesting problems but because more than anywhere else his passionate love for forests and mountains was gratified. He explored every nook and corner of it and finally built himself a home in the depths of the redwoods of Marin County near the village of Lagunitas. Everything pertaining to California then received his attention. He associated himself with the Sierra Club and his friendships extended to such as Carlos Hittell, the artist; his father Theodore Hittell, the historian; William Keith, another artist; and John Muir, the literary naturalist.

During his travels in California he had many contacts with the numerous dwindling tribes of Indians in the state. Many of these were represented only by a single family or in some cases by a single aged individual. Their pitiful economic condition excited his warm sympathy and he often befriended them. Inevitably his interest took the constructive turn when he realized that they possessed many important secrets which would die with them unless salvaged at once. Thereafter they became almost if not quite the leading interest of his life. At a fairly early date he had collected various examples of Indian handiwork, especially basketry, and eventually his collection of Indian baskets became a large and valuable one. It was kept in the spacious study of his home where it gradually dominated all ordinary furnishings from floor to ceiling and where individual pieces at gatherings of his friends often furnished the text for choice anecdotes or long fascinating folk tales which he never tired of repeating.

Friendships

Aside from his achievements as a naturalist, Merriam was distinctly a personality. Few who knew him failed to realize that he was something beyond the ordinary. He swept people

along with his own enthusiasms to such effect that only carping or jealous critics thought of him as egocentric. He was in fact very warm-hearted, very generous and very sympathetic, but without his respect these qualities were not too greatly exercised. He was not very tolerant of sloth, incompetence, or insubordination but where these did not exist he was warmth itself. In the Biological Survey he occupied a pedestal, but he did not pose, for he detested insincerity. There was a certain indefinable magnetism about him which caused men of his own or even greater stature to be drawn to him quickly. His friendships among the great and near great were remarkable. Washington in the nineties and for some years after was a delightful place. A charming social cohesion existed among the personnel of the various scientific bureaus and the national cultural institutions. The relations were simple and unconventional but full of warmth and affection. Merriam readily became a part of this and his enthusiasm for his friends was scarcely less than for some of his projects. Outside of Washington his personal relations extended to every important naturalist of his generation. Among those with whom he was especially intimate in Washington were the geographers Henry Gannett and Marcus Baker, the geologist Grove K. Gilbert, the botanist Frederick V. Coville, and the zoologists William H. Dall and Frederick A. Lucas. All these, it should be noted, were men of simple tastes and distinguished accomplishment in their respective fields. His admiration of and affection for G. K. Gilbert was especially significant, for in all the large circle of scientists in Washington at that time no man was more respected for both character and accomplishment than Gilbert. When Gilbert lost his wife, he accepted Merriam's invitation to occupy rooms in his house and literally to become a member of his family, an arrangement which was happily carried out for nineteen years until Gilbert's death in 1918. Merriam's tribute to him published shortly after (Sierra Club Bull., vol. 10, no. 4, 1919) is somewhat revealing as to his own character since he emphasized his admiration for some of those qualities in the even-tempered and ever rational Gilbert which he obviously lacked himself.

A long continued friendship between Merriam and Theodore Roosevelt led to numerous contacts and interesting incidents. As a youth in New York, Roosevelt had greatly admired Merriam's book on "Mammals of the Adirondacks" and the two were soon brought together. At that time, Roosevelt had serious thoughts of a career as a naturalist and, although he soon gave them up, his interest was a very real one which always remained near to his heart. When he came to Washington as Assistant Secretary of the Navy he soon sought out Merriam and later when he became President the relations were continued. At one time the Biological Society of Washington was thrilled by the announcement of a program devoted to a sort of debate between Roosevelt and Merriam. At the appointed hour Roosevelt bustled to the Cosmos Club where the meeting was held with a record attendance. Merriam read a carefully prepared argument detailing results of his studies of variation in coyotes and wolves of the western United States and was followed by Roosevelt who spoke without notes at such length and with such an obvious grasp of the subject that many of his hearers were quite amazed. At a later time Merriam was invited to spend an evening at the White House and to bring with him all the local naturalists and their families to meet the British nature photographer Richard Kearton who showed a film of wild birds and other animals which for its time was unusual. When Roosevelt organized his noted African trip, after leaving the presidency, Merriam was one of his principal advisers and was responsible for the choice of a considerable proportion of the personnel of the expedition. Another association with Roosevelt was in connection with the famous discussion of "nature faking," highly publicized by Roosevelt but aided and abetted if not instigated by Merriam.

Endowment for Research

In 1910, fortune again favored Merriam and, with little or no effort on his own part, he was relieved of a situation under which he was obviously growing uneasy. The Biological Survey was gradually becoming more and more an administrative and

regulatory bureau with political angles for which he had no taste. Many of the projects which had fascinated him in early life had grown to such proportions that it was plain he could not control them. What he would have done if events had not conspired for him is conjectural, but it is safe to say he would not have been deflected from his personal desires for long. Not improbably he would have retired voluntarily, since he had a small private income and doubtless could have found means to increase it. However, at this time, certain of his friends and admirers thought to have him personally endowed for research with the hope and expectation that he would produce a great work on the mammals of North America. Among those promoting the idea were his very dynamic admirer the hunter-naturalist Charles Sheldon and very probably his still more influential friend Theodore Roosevelt. Mrs. E. H. Harriman, then widowed, was approached with the result that there was established the Harriman Trust to be administered by the Smithsonian Institution and to provide Merriam support for research of his own choosing to the end of his days. The terms were extraordinarily liberal, practically everything being left to his own decision. Unfortunately it had not been realized that the subject of this wonderful endowment had a tendency to go against the tide, in fact had almost never been known to do exactly what others expected of him. Moreover, his recently developed interest in ethnology was greater than supposed.

At the age of fifty-five, he found himself with a freedom of action beyond most men's dreams, but although he plunged into work with much fervor, it soon became evident that he had changed horses and instead of advancing toward the farther shore he was being carried downstream. He devoted himself almost entirely to field studies among the vanishing tribes of California Indians and to the accumulation of enormously detailed notes about them. This became a passion with him and there can be no doubt he sincerely believed it to be more important than anything else he could do. He particularly felt that he could apply the methods he had used in zoology to secure better results than those usually attained by ethnologists.

With one notable exception, however, mammals were neglected. This was his study of the American brown and grizzly bears, begun many years before, and only completed when he had amassed specimens (mainly skulls) to the amazing total of 1,864. His division of these into an incredible number of species and subspecies, although received with considerable skepticism, was known to be based on exceedingly careful and reliable studies and his reputation for accuracy of observation was such that no one made any serious attempt to gainsay him. What he had done, however, was to marshal his facts and label them without any effort to interpret them. This was characteristic, for he had never been much given to theory. His attitude toward controversial subjects connected with the processes of evolution was usually that of suspended judgment with the conviction in most cases that the facts were not all in. When the mutation theory of De Vries was first expounded he promptly took issue with it on the grounds that it failed to explain more than a very small part of the end results of natural evolution as he knew them. Its significance for the future and its other implications did not interest him since they were not in his field and since his own work seemed to him sufficiently engrossing and important. He never wholly relinquished a Lamarckian point of view, for his intimate knowledge of the details of adaptation and evolutionary change would not permit him to accept any experimental evidence which did not fully account for them. Most theories seemed to him premature and he did not indulge in them.

Later Life

His later years were spent in following his enthusiasm for California Indians. He maintained a home in Washington and another at Lagunitas, California, scarcely a year passing that did not see him making a westward transcontinental trip and another eastward. His constant companion was his wife, who was Miss Elizabeth Gosnell, of Martinsburg, West Virginia, formerly his secretary, whom he had married in 1886. Their younger daughter, now Mrs. M. W. Talbot of Berkeley, Cali-

fornia, for some years before her marriage was also included. An elder daughter, Mrs. Henry Abbot of Washington, remained at home, but family ties were strong and in his declining years grew stronger.

After his wife's death and after he had reached the age of 80, he remained in California most of the time and it may be assumed that his research was not very active. His death in his 87th year on March 19, 1942, followed several years in a nursing home in Berkeley, California, where he was near his daughter, Mrs. Talbot, and where he was able to walk in the sun, to receive visits from old friends and to have the care which his age and its infirmities demanded.

Conclusions

The writer of this sketch for some years stood in a relation to Merriam which was too much that of an apprentice or a valet to give him wholly heroic proportions or to fail to appreciate the warmer side of his nature. He was a most extraordinary character, dynamic, productive, and original; but he was full of contradictions. Despite his record of accomplishment and notwithstanding a certain hard-headedness, he was often impractical. When concentrated on a piece of detailed research, no one could have been more insistent on considering every shred of evidence, but in the larger affairs of his life, emotions and enthusiasms swayed him. Among those who were associated with him in subordinate positions in the Biological Survey there were some who fairly worshiped him but there were also those who could never understand him and who never ceased to be resentful of fancied or in some cases real injustices. On the other hand, his personal charm and originality, his whimsicality, and his forthrightness contributed much to his success. He could hardly be called well balanced, but his inconsistencies were to him at least always connected with the prodigious urge for the advancement of knowledge which never left him. It cannot be denied that the conduct of his later years under the Harriman Trust was a disappointment to his friends, especially among mammalogists. Nevertheless there is little doubt that

his intentions were of the best. An impression prevailed that he greatly magnified the importance of his ethnological studies and that he laid out a program for himself involving such detail that it was not humanly possible to finish it in his lifetime. He published a few papers in ethnological journals and two books of folk tales, but the bulk of his results was left in a tremendous mass of notes and manuscripts now deposited with the Smithsonian Institution.

His genius was of the kind that has the capacity for taking infinite pains. In his early work he was quick to see that his subject was shot through with false conclusions due primarily to insufficient or faulty material. It became his passion, therefore, to put it on a sound basis, to correct the errors of his predecessors, and to lay a foundation for all time. He did this and much more. Perhaps his greatest contribution to his period lay in his perfecting of methods, in the use of large series of well prepared specimens, in the persistent emphasis upon exactness of geographic data, in the demonstration of previously unsuspected importance of cranial characters in the finer divisions of mammalian classification, and in his steadfast belief in the combination of field and laboratory studies. His own production was very large and his published writings include nearly 500 titles. New mammals discovered and described by him number approximately 660. The types of 651 of these are in the U. S. National Museum; 8 are in other American museums, and one is in the British Museum. The collection of mammals which he started in his first years of government service now contains approximately 140,000 specimens, vastly more than any other collection, and all with full data and in prime condition. Contemporary opinion usually thought of him as most engrossed with studies of life-zones, laws of temperature control, and the general subject of geographic distribution, but his own secret pride was in his "Monographic Revision of the Pocket Gophers," a most exhaustive study which revealed him as a perfectionist. At the time it was published (1895), there is no doubt his intense desire was to go on with similar studies of other groups.

In the history of American mammalogy his place is a very large one; in fact, it can scarcely be judged as less than pre-eminent. What he did is scarcely less important than what he influenced others to do. He was a power in the land with a reach into posterity that will long be felt. His contemporaries familiar with his whole career are now few in number and need no reminder that his was a remarkably complicated personality. For others, his record stands, but it cannot reveal the nuances of his unique character nor the warmth of his personal relations.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

Amer. Angler = American Angler
Amer. Anthrop. = American Anthropologist
Amer. Field = The American Field
Amer. Jour. Sci. = American Journal of Science
Amer. Mus. Jour. = Journal, American Museum of Natural History
Amer. Nat. = American Naturalist
Ann. N. Y. Acad. Sci. = Annals, New York Academy of Sciences
Ann. Rept. N. J. State Bd. Agri. = Annual Report, New Jersey State Board of Agriculture
Ann. Rept. Smithson. Inst. = Annual Report, Smithsonian Institution
Bull. Nat. Hist. Soc. N. B. = Bulletin, Natural History Society of New Brunswick
Bull. Nutt. Ornith. Club = Bulletin, Nuttall Ornithological Club
Bull. Phil. Soc. Wash. = Bulletin, Philosophical Society of Washington
Bull. Torrey Bot. Club = Bulletin, Torrey Botanical Club
Bull. U. S. Fish Comm. = Bulletin, United States Fish Commission
Bull. U. S. Nat. Mus. = Bulletin, United States National Museum
Calif. Hist. Soc. Quarterly = California Historical Society Quarterly
Canadian Sci. Mo. = Canadian Science Monthly
Carnegie Inst. Wash. = Carnegie Institution of Washington
Jour. Amer. Folk-Lore = Journal of American Folk-Lore
Jour. Cincinnati Soc. Nat. Hist. = Journal, Cincinnati Society of Natural History
Jour. Mamm. = Journal of Mammalogy
Jour. Wash. Acad. Sci. = Journal, Washington Academy of Sciences
Med. Rec. = Medical Record
Natl. Geogr. Mag. = National Geographic Magazine
N. Amer. Fauna = North American Fauna
Ornith. and Ool. = Ornithologist and Oologist
Proc. Acad. Nat. Sci. Phila. = Proceedings, Academy of Natural Sciences, Philadelphia
Proc. Amer. Assoc. Adv. Sci. = Proceedings, American Association for the Advancement of Science
Proc. Biol. Soc. Wash. = Proceedings, Biological Society of Washington
Proc. Calif. Acad. Sci. = Proceedings, California Academy of Sciences
Proc. Wash. Acad. Sci. = Proceedings, Washington Academy of Sciences
Rept. Comm. Agri. = Report of the Commissioner of Agriculture
Rept. Natl. Acad. Sci. = Report, National Academy of Sciences
Rept. Sec. Agri. = Report of the Secretary of the United States Department of Agriculture
Sci. Amer. Sup. = Scientific American Supplement
Sci. Mo. = Scientific Monthly

Smithson. Misc. Coll. = Smithsonian Miscellaneous Collections
Trans. Conn. Acad. Arts Sci. = Transactions, Connecticut Academy of Arts and Sciences
Trans. Linn. Soc. N. Y. = Transactions, Linnæan Society of New York
Trans. Roy. Soc. Canada = Transactions, Royal Society of Canada
U. S. Dept. Agri. Div. Biol. Surv. Bull. = United States Department of Agriculture, Division of Biological Survey, Bulletin
U. S. Dept. Agri. Div. Ent. Circ. = United States Department of Agriculture, Division of Entomology, Circular
U. S. Dept. Agri. Div. Ornith. and Mamm. Circ. = United States Department of Agriculture, Division of Ornithology and Mammalogy, Circular
Yale Rev. = Yale Review

BIBLIOGRAPHY OF CLINTON HART MERRIAM *

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Newspaper articles and all mimeographed materials are here excluded. A search through the Index Medicus has revealed but one title by Merriam (see year 1880). One of his two daughters, Mrs. M. W. Talbot of Berkeley, has told the writer that the notes on certain phases of her father's medical experiences were arranged by him for publication in book form, but that on its way to the printer the manuscript was lost in the mail and no duplicate copy had been kept.

Assistance in finding elusive titles has generously been given by Mrs. Talbot, her sister, Mrs. H. D. Abbot, of Washington, D. C., and by Miss Isabel H. Jackson of the United States Documents Division of the University of California Library.

To Dr. Alden H. Miller, Director of the Museum of Vertebrate Zoology, the writer is indebted for certain facilities, and to Dr. E. Raymond Hall and Mr. Donald F. Hoffmeister for much helpful criticism.

* Reprinted from Journal of Mammalogy, Vol. 24, No. 4, November, 1943, pp. 436-457, with permission.

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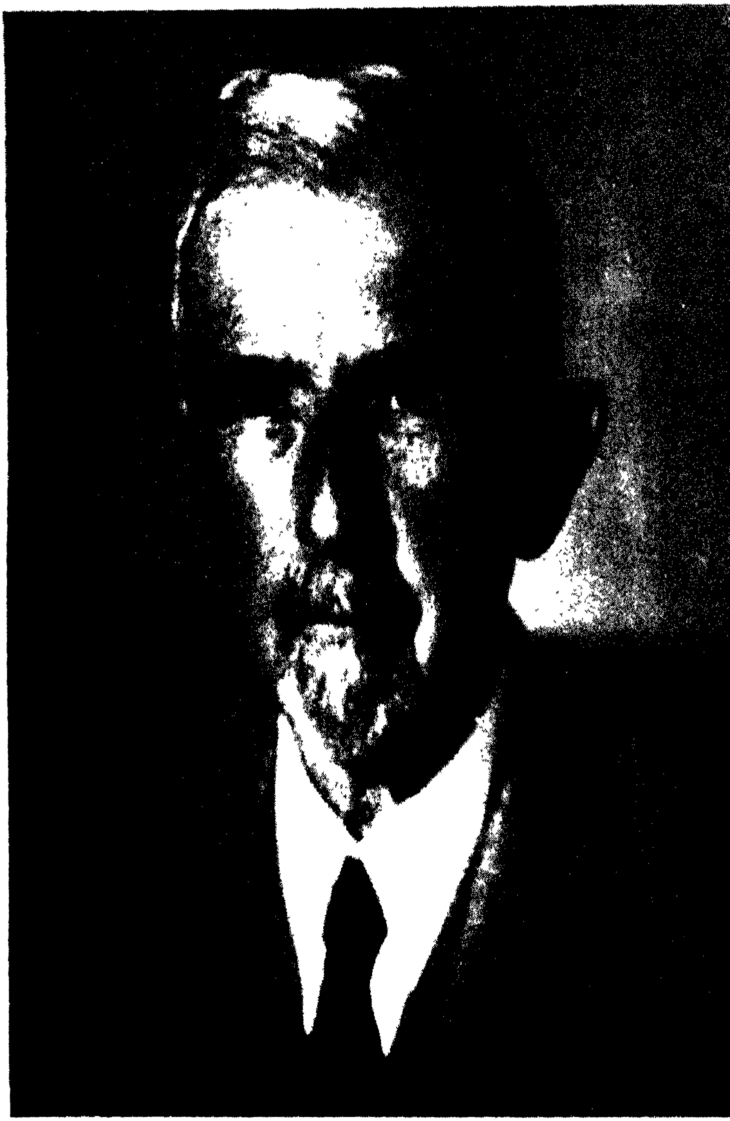

Russell H. Chittenden

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XXIV—SECOND MEMOIR

BIOGRAPHICAL MEMOIR

OF

RUSSELL HENRY CHITTENDEN

1856–1943

BY

HUBERT BRADFORD VICKERY

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1944

RUSSELL HENRY CHITTENDEN[1]

1856–1943

BY HUBERT BRADFORD VICKERY

From time to time in the field of scientific endeavor there is developed a man whose attainments, contributions, personality, and qualities of leadership are such as to dominate the contemporary scene and do much to shape the future course of human progress. A very few, the most eminent, play their part upon an international stage and in one way or another affect the lives of all, but there is a small and less widely known group to whom the common man owes a debt that he could never repay even if he were aware of its existence. To this group belonged Russell Henry Chittenden, professor of physiological chemistry in the Sheffield Scientific School of Yale University from 1882 until his retirement in 1922, and for twenty-one years thereafter the dean of American biochemistry. At the time of his death on December 26, 1943, he had been for more than fifty-three years a member of the National Academy of Sciences.

[1] A wealth of information concerning Chittenden is available from his own writings. Much use has been made of statements in his "History of the Sheffield Scientific School" and in his monograph, "The Development of Physiological Chemistry in the United States." In addition he filed a brief personal record with the National Academy, some of which has been quoted, but this information has frequently been supplemented by reference to a manuscript autobiography entitled "Sixty Years of Service in Science" prepared as he says "for my children and grandchildren" and finished in 1936. The writer is deeply indebted to Miss Edith R. Chittenden for the privilege of consulting and quoting from this comprehensive family document. The original hand-written manuscript is deposited in the Yale Library.

Use has also been made of a manuscript, "The First Twenty-Five Years of the American Society of Biological Chemists" which at the present writing is in the process of publication and for the loan of which the writer is indebted to the Editorial Office of the Journal of Biological Chemistry.

A complete set of "Studies from the Laboratory of Physiological Chemistry, Sheffield Scientific School," edited by Chittenden and formerly the property of S. W. Johnson, as well as reprints of many of Chittenden's papers, including copies of his first paper on glycogen and glycocoll and of one of the papers published during his first visit to Kühne's laboratory, are in the files of the writer's laboratory.

The advice and assistance of Dean Charles H. Warren of the Sheffield Scientific School is gratefully acknowledged.

Ancestry. Chittenden's ancestry can be traced through eight generations of Connecticut forebears to a William Chittenden and his wife Joanna Sheaffe who migrated from the parish of Marden near Cranbrook, Kent, in England, to Guilford, Connecticut, in 1639. Family tradition tells that this William had served as an officer in the Low Countries during the Thirty Years War and had attained the rank of major. Because of the persecution of religious non-conformists in England, he joined a small group of gentlemen who emigrated to Connecticut where he later occupied a prominent position in the settlement at Guilford, being lieutenant of the train-band, magistrate of the plantation, and deputy in the General Court, and was also elected fourteen times as deputy to the Jurisdiction Court in New Haven. His inventory at the time of his death shows him to have become a prosperous citizen of his adopted country.

There were ten children of whom one, Nathaniel, established a line of four successive Nathaniels all save the first of whom lived in the town of Killingworth, Connecticut. Three of the sons of the last Nathaniel, one of whom was named John, served in the army during the Revolution. John later became the father of eleven children, one of whom, Alfred Chittenden, was Russell Chittenden's grandfather. He was a farmer in Westbrook, Connecticut, whose farm, near the Clinton town line, was remembered with deep affection by his distinguished grandson. As a boy, Russell Chittenden made frequent visits to the farm and in his old age wrote an entertaining account of his early impressions of the activities such as spinning, weaving, candle making, and the preparation of soap, maple sugar, and sorghum sirup.

Horace Horatio Chittenden, the son of Alfred, was born in 1829 in Westbrook. In 1851 he married Emily Eliza Doane of Westbrook and moved to New Haven a few years later, where their only child Russell Henry was born on February 18th, 1856. The father had been educated in primary and secondary schools in Westbrook and New London and in turn taught school for a short time. Finding this unprofitable, he turned to industry and was employed in a small manufacturing

plant near Saybrook for a few years. After the move to New Haven, he became the superintendent of a plant concerned with the clothing industry. His son records that he was a man of considerable natural ability with a strong bent toward the invention of mechanical appliances, some of which were patented. He lived to the great age of 94, dying in 1923, the year after his son's retirement from the directorship of the Sheffield Scientific School at Yale.

Education. Russell Henry Chittenden left an extensive record of his boyhood and early education. He wrote that he "grew up in New Haven in a pleasant but simple home, an only child, and consequently somewhat self-centered and inclined to magnify my own importance". He was "not greatly inclined to mingle with other children, but preferred the quiet of my own home with a tendency to play alone; altogether too serious, with a fondness for books and stories, with a rather vivid imagination for a child and an ambition to be a minister or a doctor".

His education is described as follows: "At first, a small private school, followed by the public school until September 1870, when I entered a private school in New Haven to prepare for college (French's School). Languages appealed to me and were relatively easy of acquirement. Latin, Greek, French, and Spanish I studied for a time, having in mind entrance to Yale College. Mathematics I liked less but had no great difficulty with it. The only science I had at that time was a little natural philosophy. Circumstances at home were such that it was desirable for me to earn a little money. Consequently, even when in the public school, I worked most days from four to six in the afternoon in a manufacturing plant, where my father was the superintendent, running a small machine for erasing pencil marks on cards. I also had a portion of the garden for my own use and on that I raised during several years large quantities of strawberries from the sale of which I gained considerable money. At the close of the first year in French's School my parents were no longer able to pay anything on my tuition account, but when Mr. French learned that I intended to

discontinue my studies there, he proposed that I should come early mornings and dust out the school room and in addition take charge of a class of the younger boys in Latin, mathematics, and geography. This I did and thus earned my tuition during the next year to June, 1872."

Elsewhere Chittenden has recorded that Thomas B. Osborne was one of the boys he taught at French's School.

Chittenden described the steps that were taken to provide for a college education as follows: "I had begun to realize, however, that with my lack of money, spending four years in Yale College was going to be a difficult matter and two or three years additional in a professional school (for I had by then made up my mind to go into medicine) would together make a load which I could not possibly carry. It was a serious situation for me. I was loath to give up the idea of a thorough education, for, although only sixteen years of age, I realized fully that if I was to succeed in medicine I must have a proper preparation.

"Mr. French, who took great interest in me, suggested that I go to Professor S. W. Johnson of the Sheffield Scientific School for advice and he kindly went with me. Professor Johnson made it very clear that for a boy going into medicine the ideal preparation was the study of chemistry, physics, biology, etc., rather than the classical course of those days. Unfortunately, I did not have enough of the higher mathematics to pass the entrance examinations for the freshman class in the Scientific School, although I could have passed all the requirements for Yale College. Professor Johnson suggested that the best plan for me was to enter the Scientific School as a special student, devoting all my time at first to chemistry with some German.

"Consequently, in September, 1872, I entered the Scientific School at Yale, taking all the branches of chemistry offered to first and second year students and at the end of the first term gained the prize for excellence in qualitative analysis. At the close of the first year what money I had was nearly exhausted and I did not see how I was to continue. Quite unexpectedly, however, Professor Brush asked me to serve as an assistant in

the chemical laboratory for my tuition and laboratory charges so that I was able to go on, aided somewhat by tutoring backward students. Eventually, I succeeded in making up the mathematics for entrance, together with all the studies of the course, doing nearly two years' work in one, and gained the degree of Bachelor of Philosophy in June, 1875, at the age of nineteen years and four months."

Further details concerning his senior year have been given by Chittenden in his autobiography. "In those early days it was one of the requirements for graduation that the candidate present a satisfactory thesis involving a certain amount of original work, as evidence of his ability to use some of the knowledge he had gained, to think straight, and to interpret aright such data as might be collected. In a science like chemistry some problem requiring experimental work, not too difficult of solution, was usually selected on the advice of the professor, and I went to Professor Johnson for some suggestion as to what might be suitable for me to attempt. After a little thought he said that he was very fond of scallops and that when they had them for dinner any left over were usually warmed up by the cook for his special benefit and he had noticed that when served a second time they always seemed sweeter than at first. 'Now', said he, 'suppose you try and find out what there is in scallops that would explain this fact.'

"In this simple fashion the problem was set for me and I began work at once, giving to it all the time I could afford. Turning first to the literature bearing on the chemical composition of mollusks I found that practically nothing was known concerning the composition of this edible muscle of the bivalve *Pecten irradians*, that being its scientific name, so that I had a clear field. It proved an interesting and profitable study for it brought to light certain facts quite new to science, of considerable physiological significance, and incidentally it explained Professor Johnson's experience."

Chittenden prepared an account of his experiments which appeared under the title "On glycogen and glycocoll in the muscular tissue of *Pecten irradians*" in the *American Journal*

of Science in 1875. To his surprise and delight, the paper was shortly afterwards republished in England in the *Chemical News*. Possibly because of this, Professor Johnson suggested that Chittenden should translate the paper into German and submit it to Liebig's *Annalen,* and with Johnson's help this was accomplished. As later events were to prove, this step had a most important effect upon his subsequent career, and it furnishes a striking example of the good judgment of his friend and teacher.

Of his experiences during the year when, still an undergraduate, he was placed in charge of the teaching of physiological chemistry, Chittenden writes: "My services as assistant in the chemical laboratory had given me some experience and also a certain amount of courage so that I started in determined to justify the confidence that apparently had been placed on my ability to carry the work. There were six students taking the course, which extended through one-third of the college year, with five laboratory periods of three hours each week, and one recitation a week. I have had many tasks in life that called for all the strength and determination that were in me, but never before or since have I had quite such a feeling of apprehension as remained with me for many weeks as I started in on this new adventure. To be sure, there were available the advice and support of Professor Johnson, but he was a very busy man whose time and thought were given almost entirely to his own particular work, and I realized that the responsibility was mine and I was expected to shoulder it.

"By much special reading and by much preliminary experimentation I managed to pilot the class through the term with a reasonable degree of success, so much so that the following year the faculty arranged for the construction of a fairly well equipped laboratory better adapted for experimental work and the course became a permanent fixture in the curriculum of the 'Biological Course of Studies,' preparatory to the study of medicine. At that date I had no realization we were making history, or that physiological chemistry was destined to develop into a department of biological science of the highest importance

in the discovery and explanation of many of the hitherto unknown processes of life. Today, laboratories and courses of physiological chemistry are to be found in most of our American universities and medical schools, while research laboratories devoted to work in this subject are equally conspicuous, but in 1875 the small laboratory in New Haven was the only one in this country, hence it seems proper to state that the Sheffield Laboratory of Physiological Chemistry, as it came to be called, represents the birthplace in America of this growing science."

A further word of explanation concerning this laboratory may not be amiss. It was a new departure in education primarily designed for the benefit of students who later proposed to take up the study of medicine. A few years earlier the Governing Board of the Scientific School had voted to arrange a course of studies especially adapted to the needs of these men and, as a result, a course in biology was instituted. The entire course of study included mathematics, physics, chemistry, and botany, in the first year, qualitative and quantitative analysis, organic chemistry with relation to physiology and medicine, botany, zoology, and history, English and modern languages in the second year, and more specialized topics in biology such as comparative anatomy, embryology, medicinal botany and so forth in the third and final year. By 1874, the importance of the course was becoming recognized and its scope was widened. One of the steps taken was the appointment of Chittenden as an assistant in physiological chemistry, a post that he filled for several years.

Study in Germany. Chittenden soon came to appreciate the need of advanced education, especially of training in the techniques of physiological experimentation and a year of study in Germany was an obvious necessity. He made plans to study under Hoppe-Seyler at Strassburg and letters of introduction and other credentials from Yale addressed to the officials at that university were accordingly procured. Upon arrival in Strassburg in the summer of 1878, however, he was disappointed to find the laboratory crowded, the buildings old and shabby, and neither university nor city to have that undefinable

quality of "atmosphere" so valuable to a young and impecunious but ambitious student. The Strassburg so recently taken over by its German overlords was far from attractive. As he later wrote,[2] "Intuition is not to be wholly ignored, and I went on to Heidelberg with the feeling deep in my heart that the place where such men as Gmelin, Tiedemann, Bunsen, Kirchoff, Helmholtz, and Kühne had worked should give inspiration and opportunity, and that there would be found an environment more in harmony with my needs. The situation was somewhat awkward, but assuming a confidence that was not wholly felt, Kühne was sought and the hope expressed that a place might be found in his laboratory. His reception was a gracious one, but it was not difficult to see that the young American, with his imperfect command of German and with his lack of the customary credentials, presented a case quite out of the ordinary, and the outcome seemed dubious. It was explained that all necessary credentials would be forthcoming from America as soon as possible, but that did not seem to interest Kühne particularly. He appeared more interested in the visiting card he held in his hand, and much to the writer's surprise he said, 'Are you the Chittenden who published in Liebig's *Annalen* a year or two ago an article on glycogen and glycocoll?' . . . Going into his library, he came back with the volume of the *Annalen* containing the article and commented on the fact that glycocoll had never before been found free in Nature and that the presence of such large amounts of glycogen in an invertebrate muscle was an interesting and suggestive observation. The atmosphere was completely changed, and my spirits rose accordingly, reaching a still higher level when Kühne remarked that he would find a place for me in the laboratory at once. . . . After a month in the laboratory, Kühne asked me if I would like to serve as his assistant in the lecture demonstrations. Naturally, this came as a great surprise, but was accepted with alacrity, for while in a sense it was an empty honor, it led to a certain standing in the laboratory and, what was of greater importance, the very definite advantage of viewing close at

[2] "Development," p. 30.

hand all operations and experiments, in the carrying out of which Kühne was a master hand."

Besides attending Kühne's lectures in physiology, as well as those of Bunsen in chemistry, and occasional other lectures in anatomy, surgery, and pathology, Chittenden carried out three separate researches during the year at Heidelberg. Two of these were histo-chemical studies respectively on the sarcolemma of the frog sartorius muscle and on the epithelium of the eye and the third was on the formation of hypoxanthine from albumin. He had the pleasant experience of finding that, although his knowledge of physiology was elementary, his fundamental training in chemistry was far superior to that of his German, Russian, and British colleagues in the laboratory. The invitation he received to assist at lecture demonstrations probably came from Kühne's recognition of this fact. Furthermore, Kühne took pains to see that his gifted American student should meet distinguished visitors to the laboratory and often invited him to luncheons or dinners that he gave for them. Thus he met such men as Hugo Kronecker, Carl Voit, R. Heidenhain, Alexander Schmidt, Sir John Burden-Sanderson, Sir Michael Foster, Sir Henry Roscoe and Sir Lauder Brunton, and formed relations with some of them which were followed up in later years.

He also took occasion to visit many German laboratories and thus came into contact with Fresenius, both father and son, with Pflüger, Ludwig Drechsel, and with du Bois-Reymond, and his later distinguished student Baumann. Perhaps the climax was a visit to Göttingen where he met the elderly Wöhler who was a living link with Berzelius and the earliest days of organic chemistry. As he says, "The memories of these active workers, all leaders in their respective fields, have been of great value all through my life, for they have enabled me to visualize in a manner I could not otherwise have done the personality back of the scientific investigation that came from their laboratories as the years progressed."

Early Years of Teaching and Investigation. When Chittenden returned from his studies at Heidelberg in the fall of 1879, he

again took up his teaching in the tiny laboratory at Yale. At the spring commencement in 1880 he was awarded the degree of Doctor of Philosophy, the first such degree in physiological chemistry given by an American university. The requirements were evidently far less formalized in those days than they later became. Chittenden's dissertation consisted of the paper on glycogen and glycocoll in the muscle of the scallop published in 1875, and another paper published the following year on the oxidation of glycogen with bromine and with silver oxide. Whether or not any special examinations were held he does not record.

Chittenden's appointment as Professor of Physiological Chemistry in 1882 at the age of twenty-five is an evidence of the impression his attainments had made on the group of strong and enthusiastic men who made up the faculty of the Sheffield Scientific School. Within a year he became a member of the Governing Board and, upon the reorganization of that body the following year with the appointment of a Director of the school in the person of Professor George J. Brush, he was elected the permanent Secretary of the Board.

The laboratory of physiological chemistry, that in 1874 had consisted of one room, by 1886 occupied the greater part of the second floor of Sheffield Hall. Here undergraduate students taking the biological course, and graduate students both of the Scientific School and of Yale College, worked side by side. In the fall of 1889 the laboratory was moved into the former residence of Mr. Joseph E. Sheffield, the great benefactor of the School, where it occupied the entire first floor. In later years, as buildings became available for the other departments that also were accommodated in the Sheffield mansion, the laboratory expanded into the whole building, where it still remained at the time of Chittenden's retirement in 1922.

In the early years, Chittenden offered a one hour full year's course in physiology, with demonstrations and experiments, an eight hour laboratory course in physiological chemistry for a half year and a short lecture course in toxicology. These courses were attended by from 35 to 40 students of the Scientific

School and sometimes by as many as 25 seniors from the College as well as by a large group of juniors. By the mid-nineties there were as many as 200 undergraduate students taking physiology.

In addition, there were sometimes 12 graduate students doing advanced work since it was Chittenden's practice to urge unusually gifted students to remain for a year or so after graduation and undertake some special problem before going on to their medical training. By 1900, 11 candidates had successfully fulfilled the requirements for the degree of Doctor of Philosophy in physiological chemistry, and most of these had served for a period as assistants in the teaching. Two of them, Lafayette B. Mendel and Yandell Henderson, who obtained their graduate degrees respectively in 1893 and 1898, became distinguished teachers at Yale, the former as Chittenden's successor, the latter as Professor of Physiology, and both later became members of the National Academy.

Although the biological course was established at the Sheffield Scientific School with the special needs of the men preparing for medicine in mind, Chittenden early held the view, and strenuously maintained it throughout his life, that physiological chemistry is a true biological science just as are zoology, botany, or morphology. In 1930, he wrote:[3] "Morphology and physiology were the two main divisions of biology, the one dealing with form and structure, the other with function. Physiological chemistry was to be considered simply as a part of physiology, having to do with the study of the chemical functions of the living organism, animal or vegetable, as the case might be. This being so there was justification for the development of physiological chemistry in a broad biological course of study that aimed to present a more or less complete picture of the phenomena of life. Moreover, the environment so provided tended to emphasize the true position of physiological chemistry as a biological subject not restricted to the necessities of any branch of applied science. To limit the study of physiological chemistry to the needs of medicine, for example, would be to defeat the

[3] "Development," pp. 36-37 and p. 324.

end in view, *viz.*, the expansion of physiological knowledge in all its varied aspects. Medicine in the end would profit most from a broad development of physiological chemistry, realizing that every new fact brought to light is in time liable to contribute something to that fund of knowledge which is of direct use, hence of practical value, to the everyday practitioner of medicine." . . . "The main point however is that physiological chemistry should be recognized and treated as a pure science unhampered in its growth by any form of application. A science, whether it be biological or physical, to undergo a well-rounded development should have perfect freedom to progress and expand in any and all directions without regard to possible applications. Applications will come fast enough as the science advances, but just so soon as a science feels the pressure of an influence tending to limit its activities to any given channel then there is danger of a one-sided development with a restraining effect upon the growth of the science as a whole. This is a danger, as the writer sees it, which threatens the broad development of physiological chemistry in this country."

With these views in mind, Chittenden rigidly maintained the standards of training in his laboratory and imbued his graduate students with his ideals of their mission not merely to serve as teachers of a branch of learning later useful to the practitioner of medicine, but as scientists and investigators in their own right in a vitally important field of intellectual endeavor.

By 1898 Chittenden had attained wide recognition as a teacher and investigator. As early as 1885 the College of Physicians and Surgeons of Columbia had notified the Director of the Scientific School that students who had prepared for medicine in the School, and later studied medicine at Columbia, were in future to be allowed six months' credit on their three-year course. This was clear evidence of the success of the courses in biological science with which Chittenden had been associated and it was followed, in 1898, by an invitation to establish a department of physiological chemistry at Columbia. Although strongly tempted by the generous offers that were made, he found himself unable to sever his connection at Yale.

As a compromise, he undertook to set up the department and supervise it for as long as might be necessary. Accordingly in the fall of 1898 he transferred a group of advanced students, among them W. J. Gies and A. N. Richards, to New York to act as instructors in charge of the laboratory and himself lectured once a week during the college year. This arrangement continued for five years, when the full responsibility was turned over to Gies.

Administrator. The year 1898 marked still another change in Chittenden's activities and responsibilities. The tiny scientific school, which was organized in 1847 with the appointment of John Pitkin Norton as professor of agricultural chemistry and animal and vegetable physiology and of Benjamin Silliman, Jr., as professor of practical chemistry, had passed through many trials. During the early years, the appointments of professors of science usually ended with the significant words, "it being understood that the support of this professor is in no case to be chargeable to the existing funds or revenues of the College." Financial worries had been largely dispelled fifty years later, however. Many gifts, and especially the bequests of Joseph E. Sheffield, had placed the school on a firm foundation; new buildings had been erected, the faculty had expanded, and the student body was growing rapidly. The attitude of the College, at first scarcely more than tolerant, had undergone a change and science was gaining recognition as an essential component of a liberal education. In 1898, George J. Brush, who had been successively student (1848), professor of metallurgy (1855), secretary, treasurer, executive officer (1872), and director (1883) of the school, resigned his active responsibilities. Chittenden was elected as his successor for the conventional term of five years, but served continuously as director until he retired in 1922. He was at the same time appointed treasurer of the Scientific School, a position he held until 1919 when the office lapsed.

Chittenden accepted these appointments with many misgivings. He grasped the fact that the School was on the brink of a wide expansion in facilities, since such expansion had be-

come vitally necessary, and he foresaw that the position of director would interfere seriously with his scientific work if it did not entail giving it up completely. This he regarded as being wholly out of the question. After much consideration he finally decided to accept the new responsibilities but with the distinct understanding that his scientific work and some teaching were to be continued.

Chittenden thus entered upon a career which, as time went on, made steadily increasing demands upon him. Nevertheless, he continued his courses of lectures in the physiology of nutrition and in experimental toxicology until the college year 1915-16; the following year the former course was offered with Chittenden and Underhill as instructors, and, in 1917, both were omitted. Subsequently these courses were taught by others.

As the administrative head of the School, Chittenden was in large part responsible for the expansion of facilities and the enlargement of the faculty that were soon brought about. In 1897 there was a faculty of 71 of whom 20 were full professors. The student body numbered 610, 105 of these being advanced students pursuing graduate studies. There were five buildings and the students had the use of the University museum and library. The buildings were, however, strictly utilitarian; there were no dormitories nor facilities for social intercourse among the students, and inevitably the School suffered by comparison with the far more lavish equipment available to the academic students whose life was centered around the beautiful campus only a short distance away. Chittenden recognized that the students were placed at a disadvantage by these conditions and immediately took steps to have them corrected. By 1903, through the enlistment of the interest of many friends of the School, Byers Memorial Hall was ready to serve as a focus of all social and religious activities, and the following year the first unit of the Vanderbilt dormitory was built. The second unit was completed in 1906.

The more pressing social needs of the students were thus cared for but the educational needs were not neglected. The

teaching of science at Yale had always been the particular responsibility of the Scientific School. It had grown from very small beginnings in the late forties and with little help on the part of the College until most fields of scientific endeavor were represented. When, on occasion, duplication of effort was threatened by discussion of the appointment to the College faculty of a professor of some branch of science, Chittenden vigorously opposed such an appointment until he had been convinced that the interests of the School were not to be encroached upon and that the students of both parts of the University should have equal opportunity to pursue their scientific studies. Duplication there was, of course, to some extent. Elementary chemistry, physical chemistry, and analytical chemistry were taught to academic students at the Kent Chemical Laboratory of the College (opened in 1888) as well as at the Sheffield Chemical Laboratory owing to the numbers of students applying for these courses. But in general the more advanced courses in science and especially those in organic chemistry and in the applications of chemistry to engineering and to biological science remained an exclusive activity of the School.

The most pressing problem at the beginning of Chittenden's administration was the provision of better class room and laboratory facilities and of equipment for training in engineering. Kirtland Hall was completed in 1903 to care for geology, mineralogy, and physiography, and the Hammond Laboratory for mining and metallurgy was opened in 1906. As time went on, still other laboratories and class-room structures were added.

Under Chittenden there was also developed a strong faculty in the humanities since it was held that a broad training was as essential to the future scientist, physician, or engineer as it was to the future banker, business man, or lawyer. English, modern languages, history, economics, and non-scientific subjects had formed a part of the curriculum since the earliest days; the purpose of the course of studies had been from the beginning to give a "liberal education with a leaning towards science, a general training rather than specialization." The main difference, as the School developed in the first decade of the present

century, was in the attention paid to the influences of man's environment upon his economic life and to the evolution of society and of industrial organization. To Chittenden, as Director, fell the responsibility for decisions on educational policy, on appointments, and on the equipment needed to bring about these changes in the academic life of the institution. His discharge of these duties and the success that attended his efforts mark him as one of the outstanding administrators of this period in American education.

Until the reorganization of the University in 1919, Chittenden was also treasurer of the School, and the administration of all of its finances lay entirely within his powers. His office collected fees, tuition charges and rentals of dormitory rooms, as well as the income from investments, paid for supplies and labor, and met the payroll for junior instructors and assistants. Monthly settlements were made with the University treasurer for the salaries of professors and higher instructors and, at the end of the year, as treasurer of the Board of Trustees of the Sheffield Scientific School, a position he had held since 1904, Chittenden balanced accounts with the University treasurer from the income available to them. He took pride in the fact that the School was financially independent. The privileges of the University library, gymnasium, and of the physics and biology laboratories which were enjoyed by the Scientific School students, were privileges that were paid for, and these fees often required a substantial fraction of the income of the School.

During the years leading up to America's participation in the first World War, increasing costs and fixed income gradually led to a situation in which recurring annual deficits were encountered. The condition was one that affected all of the schools of the University alike and fundamental measures were called for. As a result of much thought on the part of all concerned, a complete reorganization of the University was brought about in 1919 under which the teaching of the many subjects was consolidated into University departments and laboratories. Members of the several departmental staffs were assigned to that school in which their principal teaching was done and the

distinction between a professor in the Sheffield Scientific School and one in Yale College tended to disappear. The four-year course was made uniform throughout, but all undergraduates spent the first year in a single group, the common freshman year, with its own faculty and dean, and then made their choice of emphasis upon science or arts in their later studies. Graduates of the School received the Bachelor of Science degree on completion of their course of study, and post-graduate instruction was restricted to the Graduate School.

Chittenden took a leading part in the debates and negotiations that led up to this change. The final plan adopted was a compromise between what he really wished for and what was attainable. Some of the characteristics that made the School a separate entity within the University were sacrificed but the fundamental ideals were unchanged. The teaching of science was maintained at the same high level, and the final outcome was an improvement of conditions throughout the University. Before he resigned at the age of 66 to become Director and Professor Emeritus, the construction of the huge Sterling Chemistry Laboratory and of the Sterling Hall of Medicine (occupied in 1922 and 1923 respectively) had been decided upon. All of the courses in chemistry, including chemical engineering, found their home in the former, while physiological chemistry, although greatly to Chittenden's personal regret, migrated under Mendel to the Medical School campus. But even in this location, physiological chemistry was not to lose its identity as an independent discipline. Under the reorganization, students could pass at will from school to school to obtain what they needed. Thus science undergraduates of the Scientific School found their way to the Hall of Medicine where they mingled with the medical students in their classes, and graduate students in physiological chemistry managed to cover the long mile and more to the chemistry laboratory for their essential courses and laboratory work in organic and physical chemistry. The vital point that physiological chemistry was taught for its own sake, not as a branch of applied science and an adjunct

to a medical education, was rigidly maintained. Both Chittenden and Mendel insisted on this.

Scientific Work. Chittenden's more important scientific work falls into two main categories. During the early years of his teaching career, he became interested in the action of enzymes in the processes through which the food passes after ingestion. He began with studies of the diastatic action of the saliva and then continued with a long series of investigations, partly in collaboration with Kühne, of the effects of the proteolytic enzymes. This finally led to the more general field of nutrition and included fundamental investigations that prepared the way for the outstanding work of his successor Mendel. Chittenden's own studies of the protein requirement of man led to a complete revolution in scientific thought on the subject and were regarded by him as his greatest achievement.

The second main category includes his investigations in the field of toxicology. These were initiated when he was called upon to assist his teacher Johnson in connection with the analytical work involved in a study of a case of arsenical poisoning. An improved analytical method was developed and later widely applied in obtaining evidence for the courts. In turn, other heavy metals were studied and then organic drugs; this led to the long-continued investigation of the effect of alcohol on the human body and ultimately to the work on sodium benzoate and other addenda to human food carried out while he was a member of the Referee Board of the Secretary of Agriculture.

Chittenden's first paper, published in 1875, on glycogen and glycocoll in the muscle of the common scallop, has already been mentioned although perhaps sufficient emphasis has not been laid upon the true brilliance of this accomplishment. The value of his observations was fully appreciated by Kühne, who was probably the greatest contemporary authority on the biochemistry of glycogen, and it has been shown how this paper provided Chittenden with the freedom of Kühne's laboratory.

In the summer of 1882, at the invitation of Kühne, he returned to Heidelberg and began a collaboration with his former teacher that was most unusual if not unique in the history of

American science of the period. The fundamental problem was to ascertain the chemical mechanism whereby protein ingested by the animal was rendered available to the organism. The effects of the proteolytic "ferments", as they were at that time usually called, although Kühne had coined the term "enzyme" in 1878, were understood to a certain extent, but Kühne had become convinced that the protein molecule was made up of two approximately equal parts, one of which was comparatively resistant to hydrolysis either by acids or enzymes, and was accordingly called the "anti-group", while the other part that was easily decomposed by these agents was called the "hemi-group." It seemed desirable to study the nature of the products that could be isolated from digests of the chief kinds of protein in the hope that information could be obtained on the stages through which these substances pass in the animal body. The soluble products of digestion, the proteoses and peptones, were produced by tryptic digestion and these were, in part at least, diffusible substances and could therefore pass through the membranes of the digestive tract. Contemporary physiology held that the purpose of the action of pepsin and trypsin was to transform proteins into soluble and diffusible substances adapted for absorption into the circulating blood. But these transformations were not considered to be very extensive, it being the common view that any decomposition beyond the stage at which solubility had been reached would be both unnecessary and wasteful. Kühne and Chittenden undertook to find out how far the enzymatic decompositions did, in fact, go.

The plan of investigation that was drawn up called for parallel experimentation in the Heidelberg laboratory and in New Haven with exchange of all information, and for publication of joint papers. Six collaborative papers appeared in the interval between 1883 and 1890 which dealt in turn with the products that could be isolated from digests prepared from albumins of various origins, from globulins, and from myosin. The last of these papers deals with the extraordinarily resistant protein preparation called neurokeratin that was obtained from brain and nerve tissue. These papers were published in German in

the *Zeitschrift für Biologie* and, with the exception of the first, also in English in the *American Chemical Journal* or the *Transactions of the Connecticut Academy*. Meanwhile Chittenden and his students extended the investigations in New Haven to casein, elastin, gelatin, and, in the last paper of the series, which was Mendel's dissertation for the Ph.D. degree in 1893, to the crystalline seed globulin edestin.

All of these papers are notable for the care with which the various products of digestion were isolated and "purified", and especially for the high quality of the analytical work that was carried out upon the materials. Study of these products threw much light on the complexity both of the protein molecule and of the processes whereby it is decomposed in the body, and Chittenden himself in later years regarded the work as one of his most important contributions to scientific knowledge.

That these contributions have been almost entirely forgotten by present-day biochemistry is due to a combination of circumstances. The greatest single blow was doubtless the discovery of the enzyme erepsin by Cohnheim in 1901. This at once transferred interest from the diffusible peptones and proteoses to the amino acids as the significant intermediates in digestion and removed the foundation upon which much of the work had been based. At the same time, the great development of amino acid chemistry at the hands of Fischer, the suggestion of an acceptable hypothesis of protein constitution by Hofmeister, and the demonstration by Kossel that proteins could be analytically characterized by the accurate determination of the basic amino acids, together with increasing appreciation of the fundamental work of Osborne on the preparation and analysis of proteins combined to overshadow the work of Kühne and his school in Europe and that of Chittenden in this country. Chittenden had the misfortune to enter the field of protein chemistry about twenty years before the development of the techniques and hypotheses that later made this field of investigation so generously rewarding.

This is not to be understood as a criticism of his work and still less of the devotion with which it was carried out. Much

of lasting value was indeed accomplished. General appreciation of the immense complexity of the protein molecule was not to come for many years; Fischer himself thought of proteins as substances of a molecular weight of not more than a few thousand units, and valid measurements of this quantity had to wait for the work of Sörensen in 1918 and of Svedberg ten years later. What Chittenden and Kühne did establish is that enzyme digestion is a gradual process and it became clear from their studies that the intermediate products form a mixture that defied and still defies all attempts at rational analysis. They threw much light on the process of digestion in the animal body and laid a firm technical foundation for the study of the action of enzymes on proteins which in later years was to prove of the greatest value.

Chittenden himself kept up with the more modern advances in protein chemistry but he preserved his critical faculties. In his Sigma Xi lecture delivered in 1908 in a number of western universities, he pointed out clearly that an acid hydrolysate of a protein is not the equivalent in nutrition of an enzymatic hydrolysate, inasmuch as nutritive failure occurs when the former is employed as the sole source of nitrogen in the diet. It was not until Osborne and Mendel in 1914 demonstrated that tryptophane, which is destroyed during acid hydrolysis, is an essential amino acid in nutrition that this observation received its explanation.

Parallel with the early work on the digestion of proteins, a series of investigations was carried out in Chittenden's laboratory on the enzymatic digestion of starch. The first paper appeared in 1881 and publications were submitted at intervals for several years thereafter. Much attention was paid to the effect of the addition of acid and of alkali to the digesting mixture and a brief glance at these papers shows how close the authors came to an appreciation of the effects of acidity on the action of enzymes. Beyond a certain point progress was indeed impossible in the absence of a rational theory of acidity, but this was not to come until a generation later.

During the last decade of the century, a great deal of time was occupied in Chittenden's laboratory in the study of the alcohol problem. The work was initiated as part of the investigations of the "Commitee of Fifty" which had been organized because of the widely held view that the compulsory teaching in the public schools of what was termed "scientific temperance education" was, as one writer put it, "neither scientific, nor temperate nor instructive." This group, originally organized in 1893, included leaders in education, in the church and in business, and numbered such men as Bowditch of Harvard, Atwater of Wesleyan, Welch of Johns Hopkins as well as Chittenden who, together with a few others, were given charge of the physiological and pathological investigations.

The studies carried on in Chittenden's laboratory were finally summarized in a two-volume publication sponsored by the Committee and published in 1903 under the title "Physiological Aspects of the Liquor Problem." This was a fundamental contribution to the knowledge of the effects of alcohol and was calculated to provide scientific evidence in a field that had previously been largely dominated by prejudice.

Even more significant, however, were the studies on the protein requirement of man which were begun at this time—investigations which led to a complete revolution in common thinking on the subject and gained attention and comment, some of it far from friendly, throughout the intellectual world. These studies had a somewhat unusual origin. Through correspondence with Sir Michael Foster in 1902, Chittenden had learned of an American, Mr. Horace Fletcher, for years a resident of Venice, who had been the subject of scientific study in Europe because of his claims that health could be maintained by slow and deliberate eating with particular attention to mastication. Fletcher had been examined in a number of laboratories, including that of Foster, and Foster now suggested that studies in Chittenden's laboratory might be initiated. Accordingly Fletcher was invited to come to New Haven. He was a man of broad culture and considerable wealth, an enthusiast concerning his health habits and most cooperative in the matter of

physiological tests. Chittenden entertained him in his home for many months making close observations, and soon noted that the intake of protein was remarkably low. Nevertheless, although a middle aged man, Fletcher could compete to advantage with young athletes in the gymnasium, and was manifestly in excellent physical condition. In his autobiography Chittenden wrote of him: "Considering his years, he was on the whole a remarkable exhibit, and not being versed in physiology to any extent it was natural perhaps that he should ascribe his fine physical condition to some hypothetical deglutition center, which was to be considered as a normal safeguard of health. There was no ground for belief in the existence of a center controlling mastication and deglutition in the sense in which he used the term, so that to me the chewing business became unimportant, except in so far as it tends to diminish the craving for food and thus results in the appetite being satisfied by a small amount. Hence to me the center of interest shifted at once to the question, how much do we really know as to the amount of food the human body requires to meet daily needs under the different conditions of life, especially of protein food? Excessive chewing of the protein constituents of the food certainly could have little effect on their ultimate utilization and hence could have value only in reducing the amount consumed. These were the thoughts that led to my planning the investigations."

Chittenden enlisted governmental and other aid. A detachment of volunteers from the Hospital Corps of the United States Army was detailed to New Haven, together with a physician officer in charge, to act as the subjects of the experiment. Grants were procured from the Carnegie Institution of Washington, the Bache Fund of the National Academy of Sciences, and many private donations were received, including a substantial one from Mr. Fletcher.

Meanwhile Chittenden had been warned by medical friends of the possible harmful effects of the dietary experiments he contemplated, and accordingly took a step which shows his entire confidence in the work he was about to do. A year in

advance of the beginning of the elaborately planned experiment, he himself went on the low protein diet he proposed to use. He recorded that his weight dropped from 143 to 127 pounds over a period of seven months, but at that point appeared to reach equilibrium. He recovered from a rheumatic trouble in the knee joint which had failed to respond to previous treatment, and minor difficulties such as bilious attacks and headaches disappeared. His power of endurance increased if anything and, by the fall of 1903 when the main experiment was to begin, he was convinced that no danger was to be apprehended. During this period his protein intake had been about 40 gm. daily, instead of the 118 gm. called for by the Voit standard.

The final results of the experiment were presented to the National Academy of Sciences and were published in book form in 1904. The men had been maintained in perfect health with a marked increase in physical powers for five months on a diet that yielded 50 gm. of protein and 2500-2600 calories per day.

The later extension of these experiments, carried out with dogs, gave support to his views that the essential level of protein intake is far smaller than had hitherto been supposed, but many observations were recorded of failures when highly restricted diets were employed. The problem was thus more subtle than it appeared at first, and a full explanation was not obtained until the doctrine of essential amino acids had been stated by Osborne and Mendel in 1914 and the existence of vitamins had been recognized.

In 1908, Chittenden was summoned by President Roosevelt to act, together with Doctors Ira Remsen, J. H. Long, A. E. Taylor, and C. A. Herter, as a Referee Board to aid the Secretary of Agriculture in enforcing the Food and Drugs Act of 1906. The duties of this group were to consider scientific questions referred to them by the Secretary and to make reports of their findings. They were to act independently and entirely according to the results of their enquiries without consideration of the effects upon any vested interests.

The first question proposed to them in 1908 was the hotly debated one concerning the safety of the use of benzoic acid or its salts as preservatives in food products. The experiments carried out in New Haven, in which human subjects received daily doses of benzoates far in excess of the quantities likely to be encountered in any food offered for sale, showed that there were no deleterious effects. These experiments were confirmed in their entirety by parallel ones carried out independently by Herter and by Long, and the results were published as Report No. 88 of the U. S. Department of Agriculture.

This report attracted the widest attention and was violently attacked in many quarters. The duties and powers of the Board were completely misunderstood by the press and many attempts were made to discredit this body, although without significant effect since the Secretary ruled that, in view of the scientific results, the use of sodium benzoate as a preservative is permissible in foods provided that the container or package is plainly labeled to show its presence and the quantity included.

Later questions of the Secretary involved studies of the effects of saccharin, of the practice of using sulfur dioxide or sulfites as preservatives, especially in the dried fruit industry of California, of the admissibility of aluminum salts in baking powders, and, finally, of the practice of using copper salts to improve the green color of certain vegetables, especially canned peas. Each of these matters was attacked experimentally with the most thorough attention to detail and to the practical problems involved. Each report was used in drawing up the final official decision upon the question and, when the Board resigned in 1915, they received the thanks of the Secretary for their "skillful and faithful service." Chittenden recorded his reactions to these labors in his autobiography: "Only one who has knowledge of, and experience with, the methods of chemico-physiological research required in such lines of investigation as the Referee Board carried through during the seven years of its existence can have a full appreciation of the extent of the burden and the degree of responsibility the several members of the Board carried during that somewhat hectic period. The authori-

ties at Washington, from the President down, were most kind and considerate in their treatment of the Board; we were made to feel that we were performing a most useful service, of value to the people of the United States and to other countries as well, since pure food laws everywhere, if justice is to prevail, must be predicated upon accurate knowledge and not based on preconceived notions and faulty reasoning. The Referee Board, following the precept of Marcus Aurelius, were in 'search after truth by which man never yet was harmed, but he is harmed who abideth on still in his deception and ignorance.' The Board had the satisfaction of knowing that in the course of their efforts they had uncovered truths of several kinds, by which deception and ignorance could be swept away."

War-time Experiences. During the years leading up to American participation in the first World War, the problem of procuring the necessary foodstuffs for export to England, France, and Italy became increasingly difficult. The calculations of food needed by these countries were based upon certain assumptions regarding the essential calorie intake and one of the grave problems, which became the more acute as the success of the German submarine warfare increased, was the extent to which, in view of the scarcity of food, a general economy in nutrition could be carried without danger to health. This and other related problems which demanded solution led to the formation of the Inter-Allied Scientific Food Commission composed of eight members, two each from the three chief European allies and Chittenden and Professor Graham Lusk of Cornell Medical College as delegates from America. The first formal meeting of this group was to be held in Paris, but Chittenden and Lusk proceeded to England in February 1918 where, after a stormy and unpleasant passage of twenty days under the worst of war conditions, they met with the authorities of the British Food Ministry.

It soon developed that there was going to be strong opposition to any substantial reduction in the ration of meats and fats in Britain, a ration based upon the assumption that 4000 calories per day were required by the average man, and Chit-

tenden and Lusk found that their view that 3000 calories or less were all that were really essential was decidedly unpopular. Another serious difficulty was that breadstuffs were not being rationed in Britain inasmuch as the authorities had apparently adopted the principle that the diverse energy requirements of different individuals could most efficiently be made up if bread were unrationed. Accordingly, in spite of the shortage of shipping space, large importations of cereals were called for in addition to huge quantities of meat.

Every facility was given to the two Americans to observe the workings of the rationing system and to see the provisions made for the feeding of workers. Conferences were held with officials of the Food Ministry and with the Food Committee of the Royal Society. On one occasion, Chittenden was asked to preside at a large public meeting at the Royal Institution, in the place of Lord Rhondda who had fallen ill, and took occasion to point out the efforts that were being made in America to economize on food in the effort to provide enough for the Allies.

During their stay in London, the American representatives also experienced several air raids and finally learned to take these philosophically and stay in their hotel rooms. Before proceeding to Paris for the first official meeting of the Commission, they had come to the conclusion, on the evidence offered by the members of the Royal Society Committee as well as by others, that the working people of England were as well if not better fed than they had been in peace time, and that nutritional disaster was not seriously to be feared even if the ration were to be reduced to a level commensurate with the Americans' views of the true requirements of food for the average man.

They arrived in Paris on March 22, the day before the first shells from "Big Bertha" were fired from behind the front 70 miles away, and witnessed the bitterness which this random bombardment aroused in the people. They were impressed with the increased strictness of the rationing of food in France, as compared with Britain, and especially with the rationing of bread. On March 26, the first meeting of the Commission took

place and again they detected in the opening addresses the anxiety regarding the dangers both social and military that might arise from a possible scarcity of food. The debates centered around the fundamental problem of the calorie requirement of an adequate diet. The American representatives presented data to show that a total intake per person might well be as low as 2300 calories per day and still meet all physiological needs, although supplementation to a somewhat higher level should be allowed for the heavy worker. Chittenden supported these views with data on food consumption in Germany as well as from his own experiments. The French delegates were prepared to accept a low minimum ration as the basis of computation of food requirements, but the British delegates were obviously concerned with the possibility of social unrest if serious restrictions of food imports were recommended by the Commission. After much debate, a compromise was finally reached which provided for 3300 calories "as purchased" as the basis for the average man doing average physical work, with the express statement that a reduction of 10 per cent might be borne for some time without injury to health should such a reduction become necessary. Other questions debated concerned the allowances for the different categories of the population and the vital problem of the best use of cereals.

After sessions lasting for a week, the Commission adjourned for a month when they planned to meet again in Rome. These later meetings were devoted to the consideration of the composition of the population so that the factors could be computed with which to express the needs of an actual population in terms of the hypothetical "average man", and to the exact quantities of foodstuffs that must be imported to provide for these needs. Throughout, Chittenden and Lusk stressed the desirability, for the civilian population at least, of living at a lower level of basal metabolism than was customary under normal conditions, and repeatedly pointed out that experiment had shown that this could be done without sacrifice of the capacity to work and without danger to the organism. In this they were at the same time fulfilling their functions as physi-

ologists interested in promoting the newly ascertained facts of human nutrition and as representatives of the country that must produce and export a large share of the food that was to be used by the allied peoples.

The final sessions of the Commission were held in London in June. Statistical details of crop production and food requirements both in America and in the allied countries were considered and policies were elaborated in preparation for the final reports. Out of the deliberations came the conviction that national laboratories for the study of human nutrition should be established in which the problems that arise in connection with the food supply of each country should be the subject of investigation, since it had become clear that there was insufficient definite information available anywhere concerning the most economical use of foodstuffs either for the feeding of man or of animals.

The last weeks in London were also occupied with many social activities of which perhaps the most interesting was an invitation to both of the American representatives to dine with the Royal Society Dining Club, the organization from which the Royal Society itself grew. The charter granted by King Charles II in 1662 was the formal recognition of a group of distinguished men whose habit it had been for several years to dine together occasionally for the purpose of discussing science. The membership was limited to forty and the club had a continuous history from its inception. Chittenden investigated the records and found that up to 1902 only six Americans had been invited dinner guests of the club, the first of these being Benjamin Franklin in 1757. The others had been Alexander D. Bache, Franklin's grandson and later the first president of the National Academy, in 1837, Louis Agassiz in 1840, Arnold Hague in 1880, George J. Brush in 1886, and Alexander Agassiz in 1898.

Editor. A discussion of Chittenden's contributions to science would be incomplete without reference to his activities as an editor. At the very beginning of his career at Yale, he was invited by Remsen to prepare for the *American Chemical Journal* reports on the state of knowledge of physiological

chemistry. This involved the complete review of the current literature and the discussion of its bearing, and the publication of these reports enabled Remsen's journal to fulfill the function of present-day abstract and review journals at least to some extent.

As research papers by Chittenden and his students began to accumulate, these were collected, and since many were printed in the *Transactions of the Connecticut Academy*, a journal published in New Haven, it was easy to have them paged separately and bound for convenient distribution and exchange. The edition printed was about 200. The first volume appeared in paper covers in 1885 under the title "Studies from the Laboratory of Physiological Chemistry, Sheffield Scientific School of Yale College, for the year 1884-85", the second volume in 1887, and a third in 1889. Some of the papers included in these volumes were in fact reprinted, the originals having been first published in the *American Chemical Journal*, the *Zeitschrift für Biologie*, or the *Journal of Physiology*. The issue of the publications of the laboratory in this form was then suspended, but the practice of binding together reprints of papers from the laboratory was taken up in 1904.

During the years 1885 and 1886, Chittenden served as the American associate editor of Maly's *Jahresbericht über die Fortschritte der Thierchemie* and his name is found on the title page of Volume 15 for the year 1886. In 1890 he became an associate editor of the *Journal of Physiology* and served until 1902, his name appearing on the title page for the last time in Volume 27. In 1886, he was asked to revise the definitions in general biology, physiology, and physiological chemistry for a new edition of *Webster's International Dictionary*, a task to which he returned in 1896 when still another edition of this dictionary was in preparation. Also in 1896, he was asked to be one of the original associate editors of the *Journal of Experimental Medicine*, first published in that year under the editorship of Dr. W. H. Welch; he retained his connection with this journal until 1905.

When the *American Journal of Physiology* was started in 1898, Chittenden was invited to be one of the editors. He served in this capacity for many years; his name appeared for the last time on the title page of Volume 24 in 1909, but he still gave occasional assistance until 1914 when the ownership of the journal was taken over by the American Physiological Society.

Retirement. Chittenden's years of retirement were happy ones. Few are so fortunate as to be able to see the principles that have been fought for through the years develop and flourish as it was his good fortune to do. He had brought back with him from Germany an ideal of teaching through doing that he made the basis of his courses, and physiological chemistry was started, literally from zero at the beginning of his teaching career, and was fostered until it grew and branched in innumerable unanticipated directions. His students carried his methods and principles to almost every other medical school in the country so that when, in 1930, his monograph "The Development of Physiological Chemistry in the United States" appeared, he was able to make out an implied argument, challenged of course by some, that physiological chemistry as represented by its present-day faculties in the medical schools of this country grew almost in its entirety from seeds planted in New Haven. This was an entertaining theme for one who had devoted his life to Yale, and Chittenden did full justice to it.

He spent many happy hours also in writing his two volume "History of the Sheffield Scientific School," published by the Yale Press in 1928. His intimate familiarity with the group of older men who were responsible for the founding of the school, as well as his dominating position in its fortunes in the later years, enabled him to embellish this work with lively anecdote and description, with authentic and often somewhat startling fact, and to present the whole in a matrix of his own hard-headed philosophy that makes it eminently readable even to those who have not attended Yale University. The development of a great institution of learning is never a smooth and continuous function of time. The grim battles that were fought

and sometimes won, sometimes lost, are fully described, and it is clear that at times the chips were widely scattered indeed.

Among the papers left at the time of his death in 1943 were two manuscripts, one of which was an autobiography which gives a full account of his early life, especially his experiences in Germany, and also of his later scientific work. The story of his experiences while a member of the Referee Board from 1908 to 1915 is especially entertaining in view of the misunderstanding of the functions of this body on the part of the general public and the abuse to which it was subjected in the press. The other manuscript is an account of the first twenty-five years of the American Society of Biological Chemists, which is to be published through the agency of this society in the near future. It contains a full discussion of the origin of the society and the story of its broad service to science from its foundation in December 1906, when Chittenden became the first president, until the meeting of 1931. It includes brief biographical notes concerning the original group of men who met to form the society and of those who subsequently were elected to the office of president, as well as accounts of the successive meetings. Particular attention is given to the somewhat unusual relationship of the society to the *Journal of Biological Chemistry* and of how this relationship developed.

Chittenden's retirement thus by no means implied cessation of activity. He retained his membership on the Board of Trustees of the School and fulfilled the duties of treasurer until June 1930. He had thus participated in the deliberations of this body for thirty years and had acted as treasurer since 1904. He frequently attended University social functions such as dinners for distinguished visitors and lectures, and on a few occasions sat with the department of physiological chemistry in the oral examinations of candidates for the doctor's degree. Here his calm wisdom and broad knowledge, together with his extreme fairness and concern for the best interests of the candidate, were most clearly shown. One of his last public addresses was made in 1936 when the friends and associates of

the late Professor Mendel gathered to do honor to the memory of his successor and former pupil.

Chittenden was a man of great personal dignity. He was short and slim, always immaculately dressed, and wore a neatly trimmed pointed black beard which became grey only late in life. His eyes were piercing and direct and had the power to transfix the guilty student or the opponent in debate in a manner seldom forgotten or twice risked. In the laboratory, he was definitely the master. He gave his directions for work to be done clearly and fully and expected and exacted nothing short of the best efforts of his students and assistants. By the same token, he also gave of his best to them. In the classroom at recitation or lecture, he dominated the scene with his forceful personality and incisive statements. His lectures were masterpieces of lucid presentation and his classes were enjoyed or feared according to the capacities or preparation of his students.

He carried these qualities of dignity and incisiveness into the meetings with the faculty or with the University boards of which he was a member. There is a story that on one occasion the late Dean Jones of the College was dared by one of the more frivolous members of the Board to address Chittenden by his first name during the meeting. Chittenden stared at him for nearly a minute before his face finally broke into a smile.

Although perhaps difficult to get to know, when Chittenden had once given his confidence he became a lasting and loyal friend. Even those with whom he most frequently came into conflict in his administration of the affairs of the Scientific School acknowledged their entire respect and admiration and received both in return. He identified his own interests with those of the School and of its students, and ever maintained that these last were the more important. Essentially a kindly man, he was always willing to devote his full attention to the problems of the student. As late as 1940, he was approached by a graduate student who had been assigned the task of preparing an account of the work of Kühne; he gladly gave her a long interview in which he described incidents of the early days

in Heidelberg that illustrated the personality of his revered teacher, and led her to an appreciation of his broad and varied interests and accomplishments that found their expression in a distinguished essay.

Honors. Chittenden was the recipient of many honors in recognition of his eminence in his chosen field. He was elected to the National Academy in 1890 at the unusually early age of thirty-four, and at the time of his death was by eleven years the senior of the surviving members in length of membership.[4] He was a member of the American Philosophical Society, a Fellow of the American Academy of Arts and Sciences, a corresponding member of the Société de Biologie of Paris, and a member of the Société des Sciences Médicales et Naturelles de Bruxelles and an honorary member of the Medical Society of the State of New York. In 1930 he was made an Honorary Fellow of the New York Academy of Medicine. He received honorary degrees from the University of Toronto (LL.D. 1903), from the University of Pennsylvania (Sc.D. 1904), from the University of Birmingham (LL.D. 1911), from Washington University (LL.D. 1915), and from Yale (LL.D. 1922). In 1934, the Connecticut State Medical Society conferred on him the degree of Doctor of Medicine *in honoris causa,* a unique honor which had not previously been bestowed for more than a century.

He was president of the American Society of Naturalists in 1893, of the American Physiological Society 1895 to 1904, of the American Society of Biological Chemists in 1907, and as has been mentioned, served on the Referee Board of Consulting Scientific Experts to the Secretary of Agriculture from 1908 to 1915. During the war, he was a member of the Advisory Committee on Food Utilization and member of the Executive Committee of the National Research Council in 1917 and United

[4] Chittenden was an active member of the Academy for 53 years and 8 months. Only one other person, Addison E. Verrill, elected in 1872, died Dec. 10, 1926, was a member for a longer period, but Verrill was transferred to the roll of members emeriti in 1924, so that his period of active membership was a little shorter than Chittenden's.

States Representative on the Inter-Allied Scientific Food Commission in 1918.

Family. Chittenden married Gertrude Louise Baldwin of Litchfield, Connecticut, in 1877. There were three children, an unmarried daughter Edith Russell who lives in New Haven, a son Alfred Knight, who died in 1930 without issue, and a daughter Lilla Millard, who married Dr. Henry Gray Barbour of New Haven and had two sons and a daughter. She died in 1943. Chittenden's affection for these children and grandchildren is well shown by the care that he lavished upon the autobiography that he wrote for them towards the end of his life. He took this self-imposed task as seriously and conscientiously as he had any of his previous assignments, but, since there was no restraint upon his choice of material, was able to write with a freedom and charm and humor that he rarely achieved elsewhere. It is a fascinating and wholly enjoyable document.

Bibliography. The attached bibliography is founded upon a list of papers, deposited with the National Academy, which contains those items that Chittenden himself regarded as sufficiently important to be recorded. Many of these references have been verified and where necessary completed. A few items have been added from abstract journals and the list has been completed by the addition of material published since it was made out.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

Am. Chem. J. = American Chemical Journal
Am. J. Med. Sci. = American Journal of Medical Sciences
Am. J. Physiol. = American Journal of Physiology
Am. J. Sci. = American Journal of Science
Am. Med. = American Medicine
Am. Nat. = American Naturalist
Ann. = Liebig's Annalen
Boston Med. and Surg. J. = Boston Medical and Surgical Journal
Brit. Med. J. = British Medical Journal
Centr. Physiol. = Zentralblatt für Physiologie
Century Mag. = Century Magazine
Chem. News = Chemical News
Dietetic and Hyg. Gaz. = Dietetic and Hygienic Gazette
J. Am. Med. Assn. = Journal, American Medical Association
J. Exp. Med. = Journal of Experimental Medicine
J. Physiol. = Journal of Physiology
Med. Comm. Mass. Med. Soc. = Medical Communications of the Massachusetts Medical Society
Med. News = Medical News
Med. Rec. = Medical Record
Medico-legal J. = Medico-legal Journal
Nat. Acad. Sci. Biogr. Mem. = National Academy of Sciences, Biographical Memoirs
N. Y. Med. J. = New York Medical Journal
Phila. Mo. Med. J. = Philadelphia Monthly Medical Journal
Pop. Sci. Mo. = Popular Science Monthly
Proc. Am. Physiol. Soc. = Proceedings, American Physiological Society
Proc. Am. Soc. Biol. Chem. = Proceedings, American Society of Biological Chemists
Proc. Nat. Acad. Sci. = Proceedings, National Academy of Sciences
Ref. Handb. Med. Sci. = Reference Handbook of Medical Science
Studies from Lab. Physiol. Chem., Yale Univ. = Studies from Laboratory of Physiological Chemistry of Yale University
Trans. Congr. Am. Phys. and Surg. = Transactions, Congress of American Physicians and Surgeons
Trans. Conn. Acad. = Transactions, Connecticut Academy of Sciences
Univ. Penn. Med. Bull. = University of Pennsylvania Medical Bulletin
Yale J. Biol. Med. = Yale Journal of Biology and Medicine
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NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XXIV—THIRD MEMOIR

BIOGRAPHICAL MEMOIR

OF

FRANK SCHLESINGER

1871–1943

BY

DIRK BROUWER

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1945

FRANK SCHLESINGER

1871–1943

BY DIRK BROUWER

Frank Schlesinger was born in New York City on May 11, 1871. His father, William Joseph Schlesinger (1836-1880), and his mother, Mary Wagner Schlesinger (1832-1892), both natives of the German province of Silesia, had emigrated to the United States. In Silesia they had lived in neighboring villages, but they did not know each other until they met in New York, in 1855, at the home of Mary's cousin. They were married in 1857 and had seven children, all of whom grew to maturity. Frank was the youngest and, after 1939, the last survivor.

His father's death, in 1880, although it brought hardships to the family, was not permitted to interfere with Frank's education. He attended public school in New York City, and eventually entered the College of the City of New York, receiving the degree of Bachelor of Science in 1890. His aptitude for mathematical science, already evident in grammar school, became more marked in the higher stages of his education when he began to show a preference for applied mathematics.

Upon completing his undergraduate work it was not possible for him to continue with graduate studies. He had to support himself, and his health at that time made it desirable for him to engage in outdoor activities. These circumstances caused him to enter the employ of the Title Guarantee and Trust Company of New York as surveyor. In this capacity he was active during two years (1890-1892). Then followed four more years as surveyor for the Department of Street Improvements of New York City (1892-1896).

Some years earlier he had begun to look forward to an astronomical career, and in 1894 he entered Columbia University as a special student in the department of astronomy to the extent that his full-time occupation as surveyor would permit him. For two years this arrangement was continued. By that time he had shown sufficient ability and further promise in his chosen field to be awarded a University Fellowship that enabled him to enter

Columbia University as a full-time graduate student in astronomy (1896-1898).

In the year 1890 the American pioneer in celestial photography, Lewis Morris Rutherfurd (1816-1892), had presented to the observatory of Columbia University his collection of photographic plates and the records of the measurements made by him and his assistants. These plates were of unique value on account of their high quality and their early date. Under the direction of John K. Rees the observatory, now the Rutherfurd Observatory, undertook the study of many of the photographic plates of star fields in Rutherfurd's collection. During the first winter of his graduate work Schlesinger began the measurement of eight plates of the Praesepe group, the discussion of which was the subject for his doctor's thesis (2).* This thesis is a solid contribution to photographic astrometry, in which every important step in the work is presented in full detail.

His first scientific publication (1) appeared shortly before the publication of his doctor's thesis, and deals with the possibility of simplifying the corrections for refraction to rectangular coordinates on a photographic plate by absorbing the principal portions of these corrections in the rotation error and the scale error of the plate. This is an application of an exceedingly important principle in photographic astrometry that permits one to deal with numerous corrections without requiring their laborious calculation and application to individual stars. Some of Schlesinger's most important later contributions are in principle very closely related to the content of his first publication.

During the summer of the year 1898 he was enabled to work at the Yerkes Observatory as research assistant. Schlesinger already had the desire to use a long-focus telescope in an attempt to make photographic determinations of stellar distances. The project appealed to Director George Ellery Hale (1868-1938), but he did not succeed at that time in securing the necessary funds for an assistantship at the Yerkes Observatory that would enable Schlesinger to make such an experiment.

* These numbers refer to the bibliography which follows.

Having returned to Columbia University in the winter of 1898-99, Schlesinger received an offer to become the observer at the International Latitude Station at Ukiah, California, to be established as one of six stations near the same parallel of latitude (39°8′N) for accurate determinations with zenith telescopes of the variation of latitude at each station. This problem had received considerable attention during the preceding decade, due to the discovery by F. Küstner (1856-1936) of the latitude variation and to the analysis of available material by S. C. Chandler (1846-1913). Schlesinger accepted the post, and proceeded to Ukiah to supervise the construction of the pier, the telescope housing, and a little office building. The observing program began in October, 1899, simultaneously with observing at five other latitude stations. The work soon became a steady routine, requiring his almost uninterrupted presence at the observing station, and constant watchfulness that the instrument was kept in proper adjustment to yield the best possible results. The high precision of the observations at Ukiah in the summary for the first two-year period is an indication of the excellence of Schlesinger's work.

On the whole this period was a happy one. Here he met Miss Eva Hirsch whom he married in 1900. At Ukiah also their son and only child, Frank Wagner, was born in 1901.

The observing routine left him a considerable amount of spare time that he began to use for miscellaneous astronomical studies. He had become well acquainted with the astronomers at the Lick Observatory and at Berkeley, and became active in helping Dr. R. G. Aitken on the editorial board of the Publications of the Astronomical Society of the Pacific. He undertook to write frequent notes on the progress of astronomy. The most important consequence of this work was that it gave him a life-long habit of following with interest all significant advances in astronomy.

The Carnegie Institution of Washington was founded in 1902. One of the first applications for grants-in-aid was one by George Ellery Hale, supported by Simon Newcomb (1835-1909) and E. C. Pickering (1846-1919), to enable Schlesinger to carry out a series of photographic measurements of stellar parallaxes with

the forty-inch refractor of the Yerkes Observatory. The grant was made for an initial period of one year, but with the expectation that further support of the project would be forthcoming. The work at Williams Bay began in May, 1903, and soon the Schlesingers were at home in their new surroundings. Instead of the isolation of Ukiah they now enjoyed the daily companionship of the other astronomers and their families, and friendships were made that were to last for many years.

Perhaps the unhurried reflection during the four years of postponement contributed greatly to the success of Schlesinger's parallax work during the two brief years at the Yerkes Observatory. It is instructive to compare the method finally adopted and described in the series of papers (72, 73) containing the results of the parallax determinations with the Yerkes refractor with his earlier notions expressed in 1899 (7). At that time he expected to avoid the guiding error due to the difference in brightness between the "parallax star" and the "comparison stars" by desensitizing the plate in the center where the parallax star was to be exposed. At the Yerkes Observatory he developed the device of introducing a rotating sector in the center of the plate to produce an intermittent exposure of the brighter star. The superior quality of this device is evidently that it can be controlled to a much higher degree of refinement than the method of desensitization. All plates were taken near the meridian with the telescope at the same side of the pier at all times. The importance of this precaution had been recognized by J. C. Kapteyn (1851-1922). The temptation of an observer was always to permit large hour angles east and west of the meridian in order to secure plates with large parallax factors at the cost of introducing systematic errors due to differences in the colors of the stars of which the images were to be measured. Kapteyn had expressed a preference for taking on each plate exposures approximately six months apart, with the parallax star at the opposite sides of its parallax ellipse. This would have the advantage of making the parallax solution depend upon purely differential measurements. Kapteyn's suggestion was beautiful in principle since it would eliminate a considerable portion of the

plate errors. It had, however, such serious practical drawbacks that Schlesinger decided against it. With few exceptions the plates were measured in right ascension only. Since the displacement in declination adds, as a rule, comparatively little weight to the parallax determination, the omission of the measurement in declination resulted in appreciable economy at little or no sacrifice in accuracy.

An important innovation was the introduction of what has become known as the dependence method of reduction. The usual procedure had been to reduce the measurements of all the plates for a region to a common standard by the solution for each plate of a set of plate constants from the measured coordinates of the comparison stars. The residual for the parallax star obtained by the substitution of this solution is the only datum yielded by the plate that is useful in the subsequent solution for parallax and proper motion component. With the dependence method the result is obtained without going through the solution of the plate constants and with a considerable saving in calculation. Instead of a numerical evaluation of the dependences Schlesinger introduced a simple geometrical construction that is particularly advantageous if more than the minimum number of three comparison stars is used.

In 1904 Hale left the Yerkes Observatory to take charge of the establishment of the Mount Wilson Solar Observatory of the Carnegie Institution of Washington. This important event in American astronomy was indirectly the cause of a curtailment of the parallax work then in progress. Arrangements were contemplated by which Schlesinger would join the staff of the Mount Wilson Observatory and the observations and measurements for the parallax series would continue at the Yerkes Observatory.

Into the correspondence on these matters enters the directorship of the Allegheny Observatory of the University of Pittsburgh which was vacant at that time, and which was offered to Schlesinger in March, 1905. His acceptance of this position required new changes in the plans. Both Hale and the new director of the Yerkes Observatory, Edwin B. Frost (1866-

1935), were anxious to see at least a minimum parallax program at the Yerkes Observatory carried to completion. With some further financial support by the Carnegie Institution this was actually accomplished.

The results of Schlesinger's work at the Yerkes Observatory were epoch-making, the accuracy of his determinations of stellar distances far exceeding that of previous measurements by others. His procedure has since been used so universally that it is difficult to realize that it was so completely developed by a young astronomer in such a short time.

The Allegheny Observatory had had a distinguished past under Langley (1834-1906), Keeler (1857-1900), and Wadsworth (1872-1936). During Keeler's directorship plans were formed for a new observatory. These plans included an impressive observatory building in Riverview Park and its equipment with the 30-inch Keeler memorial reflector as well as a large long-focus refractor. John A. Brashear (1840-1920), maker of astronomical instruments, had been the leading spirit in the drive for funds to build, equip, and endow the Allegheny Observatory. He continued to be active as a member of the Observatory Committee until his death.

The Keeler memorial reflector was completed shortly after Schlesinger took office at the Allegheny Observatory. The telescope soon made excellent contributions in spectrographic studies of eclipsing binaries and other spectroscopic binaries having spectra that can be studied advantageously with low dispersion. The Mellon spectrograph (66), in which a single prism was used, was especially designed for this purpose by Schlesinger and Ralph H. Curtiss (1880-1929), at that time a member of the staff of the Allegheny Observatory. The prime consideration had been to make the exposures as short as possible in view of the faintness of many stars and the lack of transparency of the Pittsburgh skies.

The first three volumes of the Publications of the Allegheny Observatory, a series of publications commenced by Schlesinger, contain about forty spectrographic studies by members of the staff of the Allegheny Observatory. This was a very significant

contribution to the store of knowledge in this field. In addition Schlesinger contributed papers on the method of reduction of spectrograms (50) and on a method of deriving the elements of a spectroscopic binary from observations of radial velocities (52). In 1909 he made the important discovery of the rotation effect in the spectra of eclipsing binaries (60).

In 1910 Schlesinger attended the meeting of the Solar Union at Pasadena. He then agreed for the Allegheny Observatory to take part in a cooperative attack on problems connected with the rotation of the sun. For this purpose the mounting of the Keeler memorial telescope was used in conjunction with a coelostat and the Porter spectrograph. The general design of the instrument was due to his predecessor, Wadsworth, but Schlesinger had to superintend its erection and to design the numerous details that require attention before a new instrument is in working condition. The study of the sun's rotation was published in 1914 (93).

The plan for the Thaw telescope, a thirty-inch photographic refractor with focal length 14.1 meters, had been included in the program for the new observatory since its conception in 1897, but its construction was long delayed owing to the difficulty of obtaining suitable disks of glass. When these were finally secured, Mr. James B. McDowell (1861-1923) of the Brashear Company produced a most excellent telescope. The instrument was ready for use at last in September, 1914.

The original plan of the Observatory Committee had been to construct a visual telescope, and in this plan Schlesinger at first concurred. However, the delay opened the possibility of changing to a photographic refractor, a modification that began to appeal to Schlesinger soon after he came to Pittsburgh and, backed by some of the leading astronomers of that day, he succeeded in obtaining the approval of the Observatory Committee. The principal argument in favor of a photographic refractor was that, with the photographic plates then available, the exposure times would be about one-tenth of those required with a visual telescope with the same aperture, with or without a color screen. It is of interest to note that the significant increase in sensitivity

of the yellow and red-sensitive plates was much longer delayed than some then expected, and was achieved only during the past ten years. Even now the photographic refractor holds its place.

The method of guiding with an eight-inch visual auxiliary telescope attached to the Thaw refractor turned out to be particularly effective in the principal work of the telescope: parallax observations of the brighter stars. It enabled the observer to guide on the bright star, an advantage not available with a double-slide plate holder used for parallax work with a visual telescope. By the installation of a floating mounting of the lenses of the principal telescope the rolling of these lenses in their cells was prevented.

Beginning with September, 1914, the Allegheny Observatory concentrated heavily upon the parallax work with the new telescope which produced an average of two hundred parallax plates a month. With the limited resources available to the observatory this presented the question whether to limit the work at the telescope or at the measuring engine. Without hesitation the latter course was adopted at some sacrifice in accuracy by the use of a reversing prism in the eye piece of the measuring microscope to avoid reversing the plate and measuring it twice. A comparison between the two methods was carried out later under Schlesinger's direction, and it was found that the use of the reversing prism accounts for an increase of the probable error by twelve per cent, in good agreement with an earlier estimate.

Concentration upon astronomical problems was interrupted by the participation of the United States in the war. In April, 1917, he offered his services to the government, but he remained at Pittsburgh until early in 1918, when he became aeronautical engineer with the United States Signal Corps, in charge of airplane instruments. The following year brought no return to quiet astronomical activity. During the early part of that year he was very active in correspondence and conferences among leading American astronomers in preparation for the first meetings of the International Research Council and the International Astronomical Union in Brussels in July, 1919. He attended these meetings as one of the American delegates. In view of

his election to the presidency of the American Astronomical Society his position among the American delegates carried particular responsibility.

Immediately following these important meetings he had to consider the invitation from Yale University to become the director of its astronomical observatory. When he accepted this post he left an active and well-equipped observatory that he had directed for a period of fifteen years to take charge of an observatory that had seen little activity during the preceding ten years and that had no telescopic equipment that could compare with that of the Allegheny Observatory. Perhaps the principal reason for his going to Yale was the prospect of undertaking parallax determinations and other astrometric work in the southern hemisphere for which the University would establish an observing station at a favorable site south of the equator. The need for such an undertaking had been evident for some years, and no one was more strongly aware of it than Schlesinger. In addition to the Allegheny Observatory, five observatories in the United States and the Greenwich Observatory in England were then engaged in determining stellar parallaxes. In this manner the part of the sky well observable from middle northern latitudes was well taken care of, but no work of this kind was in progress at any observatory south of the equator.

Almost at once correspondence and examination of meteorological records was begun to determine where in the southern hemisphere the telescope should be located. The final decision was to establish the telescope in Johannesburg, South Africa. This site offered superb observing conditions, and had much to recommend itself in other respects.

The construction of the optical parts of the new 26-inch photographic refractor proceeded most auspiciously. As with the Thaw refractor, Mr. McDowell of the Brashear Company figured the lenses from computations by Dr. C. S. Hastings (1848-1932). The objective was completed in September, 1923, two months before Mr. McDowell's death.

The mechanical parts of the telescope and its mounting were constructed in the workshop of Yale Observatory, and had been

designed by Schlesinger. This plan was not altogether a matter of choice, but was adopted when it was found that the estimates submitted by leading telescope builders were far above the amount originally budgeted, a consequence of the uncertain industrial conditions and the high level of wages and prices of materials in the early nineteen-twenties.

The completed parts were shipped to South Africa, where Schlesinger had gone to locate the site, make all arrangements to provide for the housing of the telescope, and supervise its erection. The University of the Witwatersrand made available a suitable site on its grounds and, moreover, very generously built living quarters and a small office for the astronomer in charge. The telescope was housed in a narrow building with a sliding roof. This arrangement permits observations within a small range of hour angles near the meridian only. The cost of such a structure is very low compared with a dome; for the principal work for which the instrument was intended it was entirely satisfactory and even had some advantages. For other projects for which the telescope was later used the limitations introduced by this type of housing were, however, felt as a disadvantage.

Remarkably much had been accomplished during a stay of only five months in South Africa. Dr. Harold L. Alden, who had received his training as an observer mainly at the University of Virginia, arrived in Johannesburg a month before Schlesinger's departure, and remained in charge of the telescope. The observing program got under way in September, 1925, and since that date the telescope has compiled a most excellent record in the field of photographic astrometry.

The parallax program undertaken at Johannesburg was intended to be an extension to the southern skies of the work at the Allegheny Observatory. The only important respect in which a deviation from the Allegheny practice was introduced was that the plates were measured twice, "direct and reversed," instead of with a reversing prism. Altogether the program comprised 2240 stars. Of these 1323 had been completely observed, measured and published (227, 262) at the time of Schlesinger's

retirement in 1941; the work on the remaining stars being in various stages of completion.

The enormous progress that had been made in this field of astronomical investigation was brought into full light by the publication of the General Catalogue of Parallaxes. The second edition, 1935, compiled by Schlesinger with the collaboration of Miss Louise F. Jenkins, listed over 9000 stars for which parallaxes had been determined, all reduced to a uniform system. This number included about 4000 stars with trigonometric parallaxes. The field had been well explored for the brighter stars, both in the northern and the southern hemisphere. Among the important goals achieved was the establishment of a solid foundation for methods of measuring distances of more distant stars to which the direct trigonometric method is not applicable. It was the result of the joint efforts of a number of astronomers who had for years patiently made determinations of stellar distances. The number of determinations for which Schlesinger was directly responsible was impressive, but more important had been the leadership that he had provided by the development of the method and by his choice of program.

During the last ten years of his directorship of the Yale Observatory a different project replaced the parallax work as Schlesinger's first interest. This concerned the determination of star positions by photography with wide-angle cameras.

His first work in this field began in 1913 at the Allegheny Observatory. Experimental measurements on plates taken at the Harvard Observatory were sufficiently encouraging to warrant the construction of a camera specially designed for the purpose. This camera was a doublet built by the Brashear Company, the optical design by Dr. Hastings. Its focal length was 163 cm. Special efforts had been made to guard against flexure of the telescope tube, and practical methods were devised for adjusting the cell and the plane of the plate holder. The necessity for these precautions was prescribed by the large angular distances from the axis of star images near the edge of the field. From the beginning the position work with wide-angle cameras was done on photographic plates made of heavy plate

glass. Later on, sheets of plate glass selected for flatness were used.

With this camera an equatorial zone was first photographed on plates with a field 4° by 6° (155, 169), and later two zones on plates 5° by 5° between declinations +50° and +60° (170, 184). The plates were reduced with the aid of selected comparison star positions obtained by meridian circle observations at the Lick Observatory for two zones, and at the Leiden Observatory for one zone.

The principal purpose of the zone catalogues was to provide accurate proper motions for numerous stars. Unfortunately star positions determined with meridian circles before 1900 are affected by a systematic error in right ascension depending upon the magnitude. If a proper motion is obtained by comparing a recent position with an earlier position, its right ascension component will evidently be affected by the magnitude error in the earlier position. No method exists by which this effect can be completely eliminated. Schlesinger boldly applied systematic corrections so determined that for each magnitude the proper motions in right ascension would average zero. This procedure is not entirely rigorous, but it has proved remarkably effective.

With a view to the statistical use of the proper motions the generous cooperation of the Harvard Observatory in supplying the spectra of stars not contained in the Henry Draper Catalogue was of great importance. Of equal importance were the accurate photographic magnitudes for the zones north of declination +20°. These were the result of a large research project undertaken by Dr. Jan Schilt who was on the Yale Observatory staff for five years, and who continued this work at Columbia University after 1931.

As early as 1915 Schlesinger began to think of reobserving by photography the stars in the Astronomische Gesellschaft catalogues for the entire northern hemisphere. The zones observed with the Hastings doublet were the experimental beginning of the execution of this plan. Later, at Yale Observatory, he modified the project to the reobservation of the stars in the Astronomische Gesellschaft and Cordoba catalogues between declina-

tions $+30°$ and $-30°$. This change was in part due to the fact that the Astronomische Gesellschaft had independently begun the photographic reobservation of the stars in the northern hemisphere, in part to the fact that there was a particular need for star positions in a belt in the sky that contained the zodiac and for positions of stars in the southern hemisphere.

At Yale Observatory a new wide-angle camera designed by Dr. Frank E. Ross was introduced. The focal length was chosen to be 206 cm, corresponding to a scale of 100″ to the millimeter. The angular distance from the axis for which the Ross camera still gave excellently measurable star images was much greater than was the case with the camera previously used. Hence a larger field could be measured on one plate. The first application was made to the ten-degree zone between declinations $+20°$ and $+30°$ (199, 206). On plates measuring 19 by 23 inches a field of 10° by 15° was measured. Stars in the corners of these plates were 9° from the axis, yet had images of excellent quality.

The introduction of the very large plates required much new experimental work as well as a new measuring engine, large enough to accommodate plates of this size. This was built by the Gaertner Company. Its general design was the same as that of the first long-screw measuring engine designed by Schlesinger in 1906, but, on account of its size, important modifications in detail were necessary.

After the experience gained with these very large plates a uniform procedure for the remaining zones was adopted. Plates measuring 17 by 17 inches, covering an area of 11° by 11°, were used. With new Ross cameras both at the Yale Observatory in New Haven and at Johannesburg, and with very sturdy plate holders constructed from cast aluminum, the plates for the remaining zones of the project, i.e., between declinations $+20°$ and $-30°$, were obtained. Catalogues covering the zones $-10°$ to $-30°$ appeared between the years 1939 and 1943 (246, 250, 251, 259, 260, 261). They represent the best in position work on large photographic plates that has been produced; the exceptionally high accuracy of the comparison star positions, furnished by the United States Naval Observatory and by the

Observatory at the Cape of Good Hope added to the value of the catalogues.

The increase in accuracy, from a probable error of $0''.16$ in each coordinate for the first zones photographed with the Hastings camera to a probable error of $0''.11$ for the latest zones, is most significant. The excellent quality of the later Ross cameras is an important contributing factor; equally important is the circumstance that the plate solutions for the larger plates, in which terms of higher order are included, give a closer representation of the system of the comparison stars than could be achieved with plates covering a smaller area. A further advantage is that fewer comparison stars are required for a given area; hence it has become more feasible for the meridian circle observers to furnish the positions of the necessary comparison stars.

It is likely that Schlesinger's enthusiasm led him to underestimate the extent of the undertaking when he wrote, in 1914, that "the repetition of all the northern (Astronomische Gesellschaft) zones, if carried out photographically by means of a doublet, would be a task well within the powers of a single observatory". This was before the reductions for any complete zone had been made, and without this actual experience no one could make even an approximate estimate of the very large amount of work involved. In the earlier years the calculation, for every star image on every plate, of the rectangular coordinates from the right ascension and declination was done in duplicate with the aid of standard logarithmic tables by a staff of computers under the supervision of Dr. Ida Barney. For the catalogues between declinations $+30°$ and $-30°$ the burden upon the Observatory staff was diminished by the use of the punched card machines of the Thomas J. Watson Astronomical Computing Bureau. The adaptation of the calculations to the use of these machines was made by Dr. Wallace J. Eckert. It is perhaps justifiable to say that without the use of the punched card machines the project would have been too large for the Observatory.

When it became clear that the project could not be completed before his retirement he made every effort to reduce the task

remaining for the Observatory by seeing to it that all the plates were measured. It was a great satisfaction for him to know that the work on the catalogues would continue to rank first among the programs of the Observatory. A further happy circumstance was that the work was continued with Dr. Ida Barney in full charge. She had been co-author of all the catalogue volumes published under Schlesinger's direction, and had taken an important part in every phase of the work. At the time of his death there remained about 58,000 stars on the program. When completed the Yale zone catalogues will have furnished accurate positions and proper motions of approximately 150,000 stars. The value of this material for the study of the structure of the star system is very great indeed.

From an early date Schlesinger resisted the temptation, always present, to scatter his research on a great variety of subjects. He wished to contribute a maximum effort to a limited field which, in his case, meant concentration on photographic astrometry. This explains the termination of the work at Allegheny on spectroscopic binaries as soon as the Thaw refractor became available. Even in his experimental work this concentration is strikingly present. At the Allegheny Observatory he began studies on the nature of the errors in the measurement of star images on photographic plates, which included a study of refraction anomalies (57, 84). His efforts to introduce improvements in the design of measuring engines cover a long period of years. Beyond the introduction of the long-screw engine and the design of the measuring engine for very large plates his principal contribution was the development of the projection method in collaboration with Dr. Arthur L. Bennett (198). A simple optical system was introduced by which the star image and the cross wires are projected on a white surface placed at the same distance from the eye as the circle and the record sheet. The merit of the projection method is that it eliminates the principal cause of fatigue of the measurer. At no sacrifice in accuracy it became possible to have a measurer double the number of hours a day at the measuring engine.

In connection with the zone-catalogue work he began at the

Allegheny Observatory an experiment with the view of obtaining absolute positions of stars by photography. Much later the attempt was resumed at Yale Observatory by a graduate student working under his supervision, but no conclusive results were obtained. This is one of several experiments in astrometry that were taken up at various times, and that were permitted to be crowded out by the larger projects. Among these were attempts to devise a method that would permit the accurate measurement of the moon's position by photography as well as experiments with a photographic zenith telescope.

Perhaps the fact that some of these projects were left incompletely developed accounts for Schlesinger's occasional expression of regret that so much of his own energy and of the resources of the observatories that he directed was expended on projects that involved so much routine work that there was less opportunity left for experimental work than he would have desired. Such expressions were of course entirely sincere, but they fail to state the great amount of satisfaction that he derived from the efficient execution of the large programs. He took justifiable pride in the successful employment of a staff of assistants who had no knowledge of astronomy but who had learned to make measurements and computations of a routine character. That this arrangement worked so satisfactorily was due to the fact that he could rely upon competent professional collaborators for supervision. He succeeded in keeping in close touch with the work by taking upon himself some of the operations, and he enjoyed doing this as an interesting interruption of his administrative work.

Soon after the parallax work at the Allegheny Observatory began, Schlesinger felt the need for a catalogue that contained all the important information concerning the brighter stars. Work on such a catalogue was begun in 1916, but its appearance in print (183) was delayed until 1930. The Catalogue of Bright Stars was at once recognized as an indispensable reference work of astronomers; a second edition (253) in which the material was brought up to date was published in 1940.

Every subject that he had ever taken up seriously had his

lifelong interest. Examples are the variation of latitude, observation of the sun, Algol, the Pleiades, and many others. On the Pleiades he began the compilation of a general catalogue in 1898. The work progressed slowly, and little more than a beginning had been made when, at the meeting of the Astronomische Gesellschaft in Hamburg in 1913, he met Dr. R. H. Trumpler who spoke to him about a plan for an investigation of the same general nature. This led Schlesinger to invite Dr. Trumpler to a position as assistant at the Allegheny Observatory, where the latter began his important investigations on this star cluster.

Notable also was his interest in the history of astronomy, and more particularly in the astronomers of previous generations. With deep personal devotion he could speak about the great astronomers of the recent past whom he had known when they were the leaders of the science. Examples of this reverence were his placing of a bronze plaque on the house in Nyack where George William Hill (1838-1914) was born and had worked, as well as his calls on Küstner and Max Wolf (1863-1932) when he was in Germany in 1928 to attend the Heidelberg meeting of the Astronomische Gesellschaft.

It was inevitable that Schlesinger should play an important rôle in the American Astronomical Society of which he became a member in 1905. He served successively as a member of the council, vice president and president. To the highest office in the Society he was elected in 1919 at the age of 48, succeeding E. C. Pickering, and was the youngest president in the history of the Society. It was at the meetings of the American Astronomical Society that he became well acquainted with Ernest W. Brown (1866-1938), who was mainly responsible for his coming to Yale. Though the two men worked in totally different fields of astronomy, they had many scientific interests in common. Brown was a member of the department of mathematics at Yale, but he made the Observatory his headquarters, at Schlesinger's suggestion. There are probably few other examples of an informal association of this kind that worked out so well with never an incident to mar the friendship.

On an international scale Schlesinger's rôle in astronomy became equally distinguished through his activities in the International Astronomical Union. From the founding in 1919 on he was an influential member whose wisdom in committee and council meetings was thoroughly appreciated. This culminated in his election to the presidency for the three-year term 1932-35, at the conclusion of which he presided at the meeting of the Union in Paris. At the next meeting, in 1938, the first that he did not attend, his absence was felt intensely.

Through his membership in the American Philosophical Society, since 1912, and the National Academy of Sciences, since 1916, he became associated with many eminent scientists in other fields. He had a keen appreciation of significant contributions to science, even very remote from his own field of specialization. This broadness of his interests must have come as a surprise to many who knew of the strict limitation in his choice of research subjects. It was also demonstrated by the success with which he presided at neighbors' meetings, informal gatherings of astronomers in the eastern part of the United States. They began at Schlesinger's initiative, soon after he came to Yale. Most meetings, usually three a year, were held at the Yale Observatory.

He did not lecture a great deal, but when he did, he preferred to speak about a subject close to his own special fields of interest, and succeeded in lifting his audience to an appreciation of the objectives of astronomical research. He was at his best when he was called upon to speak without much preparation. In such circumstances he exhibited an admirable command of the English language, and stated his views with order and clarity. In debates he would stand his ground firmly, and never seemed to have any difficulty in choosing the appropriate tactful words.

It may be regretted that a scientist who can expound his subject so clearly has so few students. Schlesinger's activity as a teacher of undergraduate students was always very limited. For graduate students he offered a number of subjects, with astronomical photography and the theory of errors as his favorite topics. Always teaching to a small group, his lectures were not

particularly formal, but his work was conscientious and he succeeded in stimulating the initiative of his students. Especially when they were engaged in independent research, for the doctor's degree or otherwise, he would always be ready to drop his own work and concentrate with them on finding the best approach to the solution of a problem that had arisen.

This is not the place for more than a brief account of his family life. His first wife shared with him his introduction into the astronomical world. She frequently accompanied him to astronomical meetings, and made many friends there. Her interest in others was expressed in community work, and in her relation to the observatory staff and their families. This first marriage ended with her death in January, 1928. Their son, Frank Wagner Schlesinger, received his college training at Yale University, and followed his father in interest and ability in engineering. Astronomy was for him a hobby until, after some years of practical work in engineering, he became a member of the staff of the Adler Planetarium in Chicago of which Dr. Philip Fox (1878-1944) was then director. He eventually became director of the Fels Planetarium in Philadelphia and, in 1945, director of the Adler Planetarium.

In 1929 Schlesinger married the former Mrs. Philip Wakeman Wilcox of Atlanta, Georgia, and New York City. In a very short time she had made a place for herself in the community. The meeting of the International Union at Cambridge, Mass., in 1932, brought many foreign astronomers to this country, and during that year a good percentage of them were entertained as guests at the Observatory house. Perhaps no other astronomer's wife ever was introduced so rapidly into such a large circle.

During the last five years of Schlesinger's life his failing health permitted him on ever fewer occasions to enjoy the full activity to which he had been accustomed. Wishing to conserve his strength for his research and administrative work, he became more and more retiring. The summers were quietly spent at their country house at Lyme, Connecticut, which was their home after 1941. Without any apparent effort Mrs.

Schlesinger made it her first duty to provide comfort for her distinguished husband whose health was precarious during most of the two years of his retirement from academic work. He died at Lyme, Connecticut, on July 10, 1943, after a long illness, in his seventy-third year.

On November 19, 1943, Memorial Exercises in his honor were held in Strathcona Hall, Yale University, with President Charles Seymour of Yale University presiding, and President-emeritus James Rowland Angell of Yale University and Professor Henry Norris Russell of Princeton University as the principal speakers. The following characterization of Schlesinger's scientific work is quoted from Dr. Russell's address:

"His special field, the astronomy of position, is as much an art as a science. Its main problems—where the heavenly bodies appear to be, where they are, and how they are moving—may be solved in principle by simple geometrical means. Its methods have gradually been perfected until they are accurate to one part in a million or better; but the quest for such precision unveils many unsuspected errors. To distinguish between those arising from the inevitable, but accidental, imperfections of observations and those which can be got rid of only by improving the conditions of observation requires skill in applied mathematics and statistics; to find the causes of the latter demands a thorough study of every detail of the apparatus, including the psychology of the observer; to devise ways of avoiding or correcting them needs also inventive skill; and to organize a program of research so that results of the highest precision can be obtained efficiently and at a minimum cost of labor and money is a matter of engineering and economic management. In all four of these capacities Schlesinger was a master."

CURRICULUM VITAE AND HONORS

Certificate of Proficiency in Descriptive Geometry, Freshman Year, College of the City of New York, 1887.
B.S., College of the City of New York, 1890.
Surveyor for the Title Guarantee and Trust Company, New York, 1890-92.
Surveyor for the Department of Street Improvements of New York City, 1892-96.
M.A., Columbia College, 1897.
Ph.D., Columbia College, 1898.
Research Assistant at the Yerkes Observatory, summer of 1898.
Observer-in-charge, International Latitude Observatory at Ukiah, California, 1899-1903.
Astronomer at the Yerkes Observatory under the auspices of the Carnegie Institution of Washington, 1903-05.
Director, Allegheny Observatory, University of Pittsburgh, 1905-20.
Member, American Philosophical Society, 1912-43.
Vice-President, American Astronomical Society, 1912-19.
Vice-President, American Association for the Advancement of Science, and Chairman, Section A, 1913.
Collaborating Editor, Astrophysical Journal, 1913-41.
Associate, Royal Astronomical Society, 1914-43.
Member, Washington Academy of Sciences, 1915-43.
Member, National Academy of Sciences, 1916-43.
Honorary Fellow, Royal Astronomical Society of Canada, 1916-43.
Aeronautical Engineer, In charge of Airplane Instruments, United States Signal Corps, 1918.
Foreign Member, Societa degli Spettroscopisti Italiani, 1918-43.
Honorary Member, Astronomical Society of Mexico, 1918-43.
President, American Astronomical Society, 1919-22.
D.Sc., University of Pittsburgh, 1920.
Director, Yale University Observatory, 1920-41.
A.M., Yale University, 1920.
Fellow, American Academy of Arts and Sciences, 1921-43.
Vice-President, International Astronomical Union, 1925-32.
Sc.D., Cambridge University, 1925.
Valz Medal, French Academy of Sciences, 1926.
Member, Editorial Board, National Academy of Sciences, 1926-36.
Gold Medal, Royal Astronomical Society, 1927.
(First) George Darwin Lecturer, Royal Astronomical Society, 1927.
Bruce Medal, Astronomical Society of the Pacific, 1929.
Corresponding Member, French Academy of Sciences, 1932-43.
President, International Astronomical Union, 1932-35.

(First) Honorary Member, Rittenhouse Astronomical Society, 1933-43.
Townsend Harris Medal, College of the City of New York, 1935.
Officer, Légion d'Honneur, 1935-43.
Associate Fellow, Jonathan Edwards College, Yale University, 1937-43.
Correspondent, Bureau des Longitudes, 1938-43.
Ordinary Associate, Royal Academy of Sciences, Upsala, 1938-43.
Member, Board of Directors of the Gould Fund, National Academy of Sciences, 1938-43.
Professor of Astronomy and Director of the Observatory, Emeritus, Yale University, 1941-43.

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The abbreviations used are those of the World List of Scientific Periodicals, second edition.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

Amer. Astr. Soc. Publ. = Publications, American Astronomical Society
Amer. J. Sci. = American Journal of Science
Amer. Phil. Soc. Yearb. = American Philosophical Society Yearbook
Ann. N. Y. Acad. Sci. = Annals, New York Academy of Sciences
Astr. J. = Astronomical Journal
Astr. Nachr. = Astronomische Nachrichten
Astrophys. J. = Astrophysical Journal
Biogr. Mem. Nat. Acad. Sci. = Biographical Memoirs, National Academy of Sciences
Bull. Amer. Math. Soc. = Bulletin, American Mathematical Society
Bull. Nat. Res. Coun. = Bulletin, National Research Council
City Coll. Quart. = City College Quarterly
Contr. Columbia Univ. Obs. = Contributions, Columbia University Observatory
J. R. Astr. Soc. Can. = Journal, Royal Astronomical Society of Canada
Mem. Nat. Acad. Sci. = Memoirs, National Academy of Sciences
Misc. Sci. Pap. Allegheny Obs. = Miscellaneous Scientific Papers, Allegheny Observatory
Mon. Not. R. Astro. Soc. = Monthly Notices, Royal Astronomical Society
Pop. Astr. = Popular Astronomy
Proc. Amer. Acad. Arts Sci. = Proceedings, American Academy of Arts and Sciences
Proc. Amer. Phil. Soc. = Proceedings, American Philosophical Society
Proc. Nat. Acad. Sci. = Proceedings, National Academy of Sciences
Publ. Allegheny Obs. = Publications, Allegheny Observatory
Publ. Astr. Soc. Pacif. = Publications, Astronomical Society of the Pacific
Publ. Astr. Astrophys. Soc. Amer. = Publications, Astronomical and Astrophysical Society of America
Rep. Smithson. Instn. = Report, Smithsonian Institution
Sci. Mon. = Scientific Monthly
Trans. Int. Astr. Un. = Transactions, International Astronomical Union
Trans. Yale Astr. Obs. = Transactions, Yale Astronomical Observatory
Yearb. Carneg. Instn. = Yearbook, Carnegie Institution

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1899

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1902

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1911

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1912

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1915

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1916

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1917

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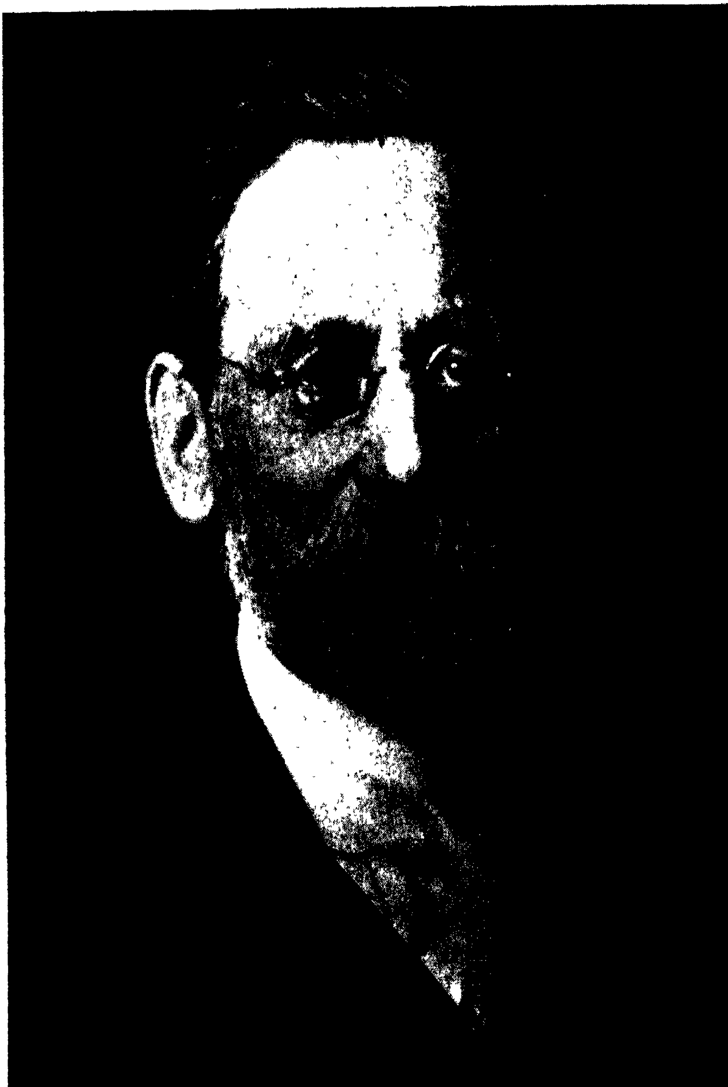

Leonhard Stejneger.

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XXIV—FOURTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

LEONHARD HESS STEJNEGER

1851–1943

BY

ALEXANDER WETMORE

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1945

LEONHARD HESS STEJNEGER

1851–1943

BY ALEXANDER WETMORE

Leonhard Hess Stejneger, for more than 58 years a member of the staff of the Smithsonian Institution, was born in Bergen, Norway, October 30, 1851, the eldest of 7 children. Records of his ancestry on the paternal side carry back to Johann Carl Steineger, born in Prague, January 26, 1726, who had as an only son Leopold August Elias Steineger born in Braunschweig about 1755. The eldest son of Leopold, Carl Claus Heinrich Steineger, born in Hamburg, Germany, March 28, 1791, became a cavalry officer, and after a period of service in the army removed to Bergen, Norway, where he established the mercantile business of Steineger & Company. He married Margaretha Stamer, born in Bergen in 1799, who, following the death of her husband in 1828, possibly from the effect of wounds received in war, continued the family business as its head. Leonhard Stejneger's father, Peter Stamer Steineger, born of this union in Bergen, December 12, 1826, was educated in private schools and in a commercial academy. He became a merchant, and later an auditor, and died in 1921.

The maternal grandfather, Leonhard Hess, a watchmaker of Bergen, was born in that place in 1789 and died in 1868. He married Maria Margaretha Brock, also of Bergen (born in 1799) who died in childbirth in 1830. Their daughter, Ingeborg Catharina, mother of Leonhard Stejneger, born February 5, 1830, was educated in a private school for girls, and married at the age of 20. She died when 45 years old.

According to records left by Leonhard Stejneger he attended the Smith Theological School in Bergen in 1859-1860, and following that the Bergen Latin School until 1869, when he accompanied his mother to Meran in southern Tyrol, Austria (in recent years included in northern Italy and called Merano). Here he was under a private tutor in 1869 and 1870. Until 1871 he spent the greater part of each year in Meran because of his

mother's delicate health. There he completed preparation for admission to the University of Kristiania, taking the "artium" in 1870, and beginning his studies in 1871 to become *candidatus philosophiae* in 1872. Following this he studied medicine for two years, mainly to have the advantage of the lecture courses in botany and zoology. As it was his mother's wish that he should become a physician he went to Berlin for further training in medicine. Almost immediately it developed that this profession was not to his liking, so he returned to Kristiania to fit himself to enter his father's business. To this end he took up law and graduated in 1875 as *candidatus juris, haud illaudibilis.* It may be added that in 1930 he received the degree of Doctor of Philosophy, *honoris causa,* from his University in recognition of his accomplishment in science.

Stejneger's interests in his chosen field of zoology developed in his boyhood. His earliest notes, which deal with birds, begin in 1867 when he was in his sixteenth year. The record in question, a form of catalog with a number at the top of each page, is on printed forms that list (in Norwegian) 33 items to be observed in specimens that he collected. These include details of place, date, sex, measurement and color, a page for general observations, and another on which he painted a beautifully executed sketch in water color. The work begins with an account of the willow warbler under the heading "Lóvsangeren, *Sylvia trochilus*" from a specimen shot at Vaaren in 1867. The second species, a dipper, was taken May 10, 1868, and from then until February 9, 1871, he treated in detail 68 species of birds, the observations beginning with No. 49, dated December 13, 1869, being made at Meran. The last two that complete this record, No. 69 and one without number, are from Kristiania, October 24, 1872, and May 3, 1874.

The sketches continue to No. 53 and then cease, all being done in careful detail, and usually in a highly lifelike manner. The earlier drawings are signed L. H. Steineger, but during his work at Meran he adopted for his family name the Norwegian spelling Stejneger, this first appearing as the signature of a painting of

Emberiza cia on January 27, 1870. This spelling he continued throughout the rest of his life.

His first publication, written in German, entitled Ornithologische Notizen aus Meran, Süd-Tirol, während der Winter 1869/70 und 70/71, appeared in the Journal für Ornithologie for March, 1871, pp. 122-124. The article is dated at Meran January 23, 1871, and consists of an annotated list of 59 species, the notes dealing mainly with abundance, with specific records for certain of the species. The statement indicates a detailed knowledge of the avifauna of the region based both on observations and on collected specimens. His second paper, Nachtrag zu den ornithologischen Notizen aus Meran, Süd-Tirol, followed in the same journal for November, 1871, pp. 462-463, giving additional records. This paper was dated "Kristiania, 15. Nov. 1871," and has at the end a statement that he was interested in the genus *Sitta,* the nuthatches, and that he would like to exchange skins and eggs of Norwegian birds for specimens of this group from other regions. Another paper, Ornithologisches aus Norwegen, which appeared in the same periodical for May, 1873, pp. 304-307, dated "Christiania (Tilestr. 39), 8. März, 1873," is based in part on manuscript material in the library of the Bergen Museum. Stejneger's first book, Norsk ornitologisk ekskursjonsfauna, a duodecimo of 111 pages and four plates, was published in Khristiania in 1873. This included a general account of birds and their study, followed by a descriptive list of 260 species of birds then recorded from Norway, with brief descriptions and statements of status. The plates depict technical details, one showing the named parts of a bird, others outlines of the bills, feet, and so on, of the different groups. Genera were listed and characterized on the even pages, and their species on the odd pages opposite, the whole being a clearly written handbook with a masterly grasp of the subject. The manual on birds was followed in 1874 by another dealing with mammals, Norsk mastozoologisk ekskursjonsfauna, a brief publication of 31 pages. Leonhard Stejneger began at an early age, therefore, to publish sound contributions to science.

Following graduation Stejneger entered his father's business

in Bergen, at the same time continuing so far as able his scientific studies. He became a member of the Deutsche Ornithologische Gesellschaft in Berlin in 1871, being inscribed on the roll [1] as Mitglied "128. Herr Stejneger, Student, in Bergen, Norwegen." Through this membership he received the Journal für Ornithologie and so established contact with others interested in birds. From his publications it appears that he had close association with the Bergen Museum, as his writings mention its collections, particularly those of birds from Madagascar taken on a Norwegian mission, on which he based several papers. This museum, founded in 1825, at the time was housed in a substantial stone building. In 1870 its records state that it had in its library 8192 items. Stejneger's first bird new to science, described in 1878, was named *Lanius bairdi,* in honor of Spencer Fullerton Baird of the Smithsonian Institution.

A letterpress book marked "L. Stejneger, Privat" for the period November 19, 1877, to May 3, 1881, deals mainly with his scientific work of this period, covering correspondence with many of the ornithologists of that day. It includes letters to such well known men as Bernhard Altum of Neustadt, Jean Cabanis of Berlin, Robert Collett of Kristiania, H. E. Dresser of London, Alfred Grandidier of Paris, G. Hartlaub of Bremen, Léon Olphe-Galliard of Angoulême, I. N. Palmén of Helsingfors, August von Pelzeln of Vienna, J. Reinhardt of Copenhagen, and Anton Reichenow and Herman Schalow of Berlin. The letters are mainly in Norwegian and German, with occasionally one in French. In addition there are numerous communications with dealers in natural history specimens regarding the purchase of skins of birds, particularly of nuthatches, shrikes of the genus *Lanius,* and swans, all of which he was studying at that time. Various orders went to Wilhelm Schlüter of Halle, others to Bouvier, Verreaux, and Deyroche of Paris, A. Frank of Amsterdam, and Charles Jamrach of London. Under date of July 24, 1880, Stejneger wrote to Cogswell and Harrison of London asking for a quotation on a "Collectors' walking stick

[1] Journ. für Orn., 1871, p. 78.

gun" that he had seen advertised in the well-known periodical The Field. And on August 31 he placed an order with them for the gun in question, 500 shells, cut wads, a cleaning rod and a set of loading tools, at a price of £2. This served him many years in collecting birds, and later reptiles, for specimens, and is today in excellent condition.

The Stejneger family, up to 1880, had been one of the wealthy families in Bergen; but Leonhard's father, through a change in the scope of the business that was the family's support, encountered financial reverses and finally went into bankruptcy. The son had to seek other livelihood and having found his business experience little to his liking decided to take up science. There was nothing available for him at the Bergen Museum, and only minor positions were open elsewhere. For a time he considered locating in Bremen, but one of his principal advisors, Jean Cabanis of Berlin, advised him to go to America since it would be years before he could expect to be considered for any of the few better class posts in Europe, while in the United States there might be greater opportunity. Stejneger, in the meanwhile, on October 30, 1876, had married Anna Normann, a school teacher of Bergen, but the two proved to be of such different tastes that they were wholly incompatible. The wife had no desire to go to the United States so they decided to separate, later securing a divorce.

Stejneger left Norway about the middle of August, 1881, and arrived in the city of Washington October 21, where he went immediately to the Smithsonian Institution. He has told me with some amusement of how he sat for a time on a park bench near the building, developing his English vocabulary by means of a pocket dictionary, before entering to call on Professor Baird, Secretary of the Institution. The two were known to one another through correspondence, and Baird, with his wide knowledge, was cognizant of the young Norwegian's competence.

Stejneger seems to have begun work without delay on the birds of the New World to which he had come. The office of the division of birds, with Robert Ridgway as Curator, was located then in the South Tower of the Smithsonian Building,

where Dr. A. K. Fisher relates that he met Stejneger in January, 1882, when the latter was working over West Indian solitaires, on which he published a paper that same spring.

Stejneger's first interest in the Smithsonian collections was in the aquatic birds, and he seems to have continued his studies on the swans begun in Bergen, as his Outlines of a Monograph of the Cygninae was published in the Proceedings of the U. S. National Museum in 1882. He also prepared an extended paper on the systematics of the American thrushes, another on the West Indian solitaires (*Myadestes*), and a fourth on nomenclatural matters, all of which were published in the volume mentioned. He had offered his services to the Smithsonian for exploration work, and there was consideration of investigations in the Hawaiian Islands and in the West Indies; but finally it was decided that the Commander Islands offered the best opportunity, partly because of the former occurrence there of the northern sea-cow (*Rhytina gigas*). This arrangement undoubtedly was influenced by the desire of the U. S. Signal Service to establish observing stations in that area. Under these auspices, therefore, with a grant of $250 from the Smithsonian for miscellaneous expenses, given to him by Professor Baird on the eve of his departure, he left Washington on two days' notice on March 22, 1882, and, after having been snowbound for several days on the Pacific Railroad en route, sailed from San Francisco April 5. The Alaska Commercial Company, which at that time held the Commander Islands under lease from the Russian Government, had offered assistance in the proposed work. Through this company Stejneger obtained transportation from San Francisco on the steamer *Aleksander II,* which carried the annual supplies to its stations. After a rough voyage the vessel stopped at Copper Island May 6, where Stejneger secured three birds, some fish and other specimens. He reached Gavan, Bering Island, May 8 where, as observer, he started the first station for the Signal Service on May 22, taking observations three times daily. At the same time he carried on his natural history work. June 16, Stejneger had opportunity to cross to Petropaulski by steamer to establish the second meteorological

station. The ship lay from June 18 to 23 at Copper Island, unable to discharge cargo because of the heavy sea, while shore work was further hindered by fog and rain. His specimen catalog carries the first entry from Petropaulski on June 27 and the last one on July 11. The Signal Service station there was organized to make two observations daily. On July 15 he was again on Bering Island, and the latter part of the summer was given to a careful study of the fur seal rookeries. Between August 21 and September 1 he circumnavigated the island. In the succeeding winter he was occupied in observing and collecting, partly near the settlements, and partly on trips with dog sledges into the interior. In May, 1883, he was again in Petropaulski to inspect the station there, and in June he made studies of the fur seals on Copper Island. He completed the work on Bering Island during the summer, and returned south, reaching San Francisco from Petropaulski on the S.S. *St. Paul*, October 26, 1883. One of his stories of this expedition related to his first attempt to handle the light skin kayak of the natives. Almost immediately he overturned and remained submerged head down, caught by the legs and unable to extricate himself. As he slowly suffocated, his thoughts were puzzled since, according to tradition, as a drowning man the events of his life were supposed to pass rapidly through his mind, but, instead, he was so intrigued by the play of light on the bottom below him through the surface ripples made by his struggles that he could think of nothing else. He regained consciousness on the beach, having been rescued and revived by his companions.

The accomplishments of Stejneger as a linguist are shown by his journals of this expedition which are written partly in excellent English and partly in Norwegian, while at the same time he was living among natives with whom he must have dealt in the Russian language.

The extensive natural history collections that he made on this trip included many bones of the sea-cow, as well as parts of the skeleton of the great, extinct spectacled or Pallas's cormorant (*Phalacrocorax perspicillatus*), a species known to the older natives in life, but which had been exterminated about 30 years

previously. The 700 birds secured formed the basis for a report of 382 printed pages, entitled Results of Ornithological Explorations in the Commander Islands and in Kamtschatka, which was published in 1885 as Bulletin 29 of the U. S. National Museum. Throughout the expedition Stejneger, in addition to help from the Alaska Commercial Company, received much assistance from the firm of Hutchinson, Kohl, Philippeus & Co., this including steamer passage and quarters during the period of the work.

On his return to Washington he took up studies in ornithology so energetically that, in addition to the preparation of the larger report just mentioned, he published numerous shorter notes and papers, mainly on birds, but including travel notes, with some observations on plants and on mammals.

Being established now in the New World, to enter more completely into the life of the country to which he had come, on July 14, 1884, Stejneger filed his application for citizenship, and was admitted to that privilege formally on February 4, 1887, when he was 35 years of age.

In the latter part of 1884 the Smithsonian Institution prepared an extensive exhibit for display at the World's Industrial and Cotton Centennial Exposition in New Orleans, the material when finally shipped filling 17 cases. The department of birds arranged a series of 163 mounted game birds, in part under the supervision of Stejneger, who was employed as an assistant for this work from July 1 to December 1, 1884. On December 1, 1884, he was appointed assistant curator in the department of birds, under Robert Ridgway as curator. The accession record for that year includes a number of gifts of birds from Stejneger, mainly from the Old World. From January 3 to 16, 1885, he was absent in New Orleans to install the collection mentioned above for exhibition.

J. S. Kingsley at this time was engaged in the publication of the Standard Natural History, and in connection with the preparation of the volume on birds asked Stejneger to submit an outline. This seeming acceptable there was immediate and pressing demand for manuscript, so that he submitted what he thought would be a preliminary draft of the proposed introduction and

the accounts of the first few orders. To his amazement this manuscript was returned to him in what seemed a remarkably short time in galley proof, with rather peremptory request for more copy. As a result he actually prepared over 360 of the 547 printed pages of this work, being forced to submit his material as rapidly as it was written with no opportunity to check it as a whole. The uniform excellence of the material brought out under such pressure is a definite tribute to the systematic mind of the author. While intended for a popular audience the careful treatment accorded by Stejneger commended the book immediately to his scientific colleagues, who, according to one reviewer, might not agree with all of the author's ideas, but as students of the subject needed to consult and study his findings. This writing was done out of office hours, as during the day he was busy with other ornithological researches.

At this period Stejneger was actively occupied with the birds of Japan, as the annual report of the National Museum for the fiscal year ended June 30, 1886, states that he "continued his revisions of Japanese ornithology." This continued during the following year, when Ridgway in his annual report records 59 papers "written by the curator and assistant curator." Stejneger also made studies of collections of Hawaiian birds.

In the fiscal year 1888 Stejneger proceeded with his investigations on Japanese birds, covering several private collections forwarded to him from Japan, in addition to the materials in the National Museum. He reported also on two collections of Hawaiian birds, and investigated several groups of European species. His work on Japan continued as a principal activity until March, 1889. In that month there came a very definite change in his scientific researches. Dr. H. C. Yarrow in charge of the department of reptiles and batrachians having resigned, Professor Baird and G. Brown Goode, Assistant Secretary, prevailed upon Stejneger to take appointment as Acting Curator effective March 1, and to begin investigations in this new field. Though he never gave up his interest in birds his investigations in herpetology soon absorbed him almost completely, and he became one of the foremost authorities in the systematics of

this branch of science. His first task was to overhaul the collections placed in his charge, to institute new methods in cataloging, and to arrange the specimens so that they were readily accessible. Before the end of the fiscal year he had completed a short paper on boiform snakes and had another in progress.

Interest in these new duties, and a thorough-going curatorial conscience that made him want to see the collections newly placed under his charge in first-class order, led to overwork for which he did not possess sufficient physical strength. By late summer of 1889 he was definitely exhausted, and had developed a bronchial trouble. The condition of his health making it necessary for him to leave the city, he went to Flagstaff, Arizona, to join a field party under C. Hart Merriam that was conducting a general biological survey of the San Francisco mountain region. When Stejneger arrived he was so weak that Merriam, a graduate physician, believed he had only a short time to live, so that he consented with great reluctance to Stejneger's insistent request that he be taken to the Grand Canyon, which he had always wished to see. The trip was made by buckboard during the middle part of September. The mountain air and sun, and a naturally strong vitality, brought immediate improvement, so that despite Merriam's pessimism Stejneger was soon about again as usual. During the two months following, he made collections of reptiles and amphibians in Arizona, New Mexico and Texas, returning to Washington refreshed and fit to resume his museum duties.

During the fiscal year 1890 he prepared an extended report on the herpetological collections made by Merriam and his associates in the Death Valley region, and also on a collection made by Merriam and Bailey around San Francisco Mountain. The collections in his department were moved from the basement to new and more commodious quarters in the top rooms of the South Tower.

The years that followed were occupied with arrangement of the collections in his department, with the addition of specimens, and with studies that led to publication of new knowledge. It may be mentioned that he continued work on Japanese birds

and on occasion on other collections concerned with his earlier investigations in ornithology. At the World's Columbian Exposition in Chicago in 1893 he had an exhibition of casts of poisonous snakes, and of turtles, supplemented by specimens preserved in alcohol.

On March 22, 1892, in Washington, he married Helene Maria Reiners (born in Crefeld, Germany), a union that proved happy and congenial in every way. In 1907 when in Kristiania they adopted an infant daughter, Inga, now Inga Stejneger Miller, employed in the service of our government.

An important publication of this period was The Poisonous Snakes of North America, a paper of 150 pages and many illustrations, that appeared in the National Museum report for 1893, actually issued in 1895. This remains a classic in its subject.

In the fall of 1894 Stejneger made a trip to the Bad Lands area of South Dakota during which he obtained various useful specimens. And in the same year he made experiments in the use of formalin as a preservative.

The problem of the fur seal herds of the North Pacific and more northern waters at this time had become both an economic and a political one, and it was natural that Stejneger with his early experience in the Commander Islands and his recognized breadth of knowledge should be a member of the commissions that investigated on behalf of the United States in 1895, 1896, and 1897. The field work involved and the preparation of reports occupied most of his time for these three years.

In 1895, as a special attaché of the U. S. Fish Commission, Stejneger left Washington May 28, bearing letters from the Russian Legation, authorizing his work. He sailed from San Francisco June 6 on the S. S. *Bertha,* and on June 23 continued from Unalaska on the S. S. *Albatross.* After stopping briefly at St. Paul Island in the Pribilofs, he reached Bering Island July 3, where the ship left him two days later. Pages of his journal are filled with observations of the changes that had come in the village since he was there in 1883, of the acquaintances that he encountered, of his dealings with his old friend, the Russian Governor, N. Grebnitski, and of his pleasure at

occupying again the old house which served for a second time as his headquarters. In one entry made on July 15 during a journey by dog sled from the village of Gavan to one of the seal rookeries (Severinij) he writes "nearly all of the snow had disappeared from the road so that our progress was slow; in fact we did not reach the rookery village until 5 p.m. To make it lighter for the dogs I walked a good deal of the distance, and a very exhilarating and delightful exercise the trip proved to be. I can stand fatigue about as well as I could twelve years ago. I am a young man yet, at least here in this climate. Most people here also wonder at seeing me so unchanged." Another comment was that his dog drivers now were the sons of those who had served him in his earlier work.

On July 27 he crossed to Copper Island in the Russian Seal Skin Company's steamer *Kotik,* and with Governor Grebnitski went around the island in a small boat, visiting the fur seal rookeries. July 30 he mentions seeing a score of sea otter mothers with their young, and the following day notes that he found them common. August 12 he returned to Bering Island and then continued to Petropaulski, Kamchatka. From here he made another trip to the Commanders in September, finally sailing from Petropaulski on the *Kotik* September 18 to reach San Francisco October 11. His first report, entitled The Russian Fur Seal Islands, which covers 148 quarto pages, he wrote in three and one half months.

In 1896 he went north again under appointment by President Cleveland as a member of the International Fur-Seal Commission. The greater part of July was devoted to the rookeries on the Pribilof Islands, after which he proceeded on the *Albatross* to the Commander Group where he worked mainly on Copper Island from July 30 to August 8. In the following days the *Albatross,* after coaling in Petropaulski, cruised through the Kurile Islands, and from August 28 to 31 stopped at Robben Island. Returning through the Kuriles he came to Hakodate, Japan, September 10, and returned finally to Washington December 22, after an absence of six months.

In 1897 in continuation of his previous year's appointment,

accompanied this time by Mrs. Stejneger, he left home June 5, sailing from Tacoma, Washington, in the S. S. *City of Topeka* June 13. June 20 they transferred in Sitka to the revenue cutter *Grant,* which carried them via Unalaska to Nikolski, Bering Island where they landed July 7. From August 16 to 31 he was on Copper Island, and then was given passage on the Imperial Russian Steamship *Yakut* for Petropaulski. Another trip was made to the Commander Islands in September, and in October the Stejnegers finally sailed from Petropaulski for Hakodate on the Japanese vessel *Taiya Maru,* arriving October 25. They continued by rail to Tokio and Yokohama, and then by steamer via Honolulu to San Francisco, reaching that city November 27 and Washington December 15.

In 1898 Stejneger made his first visit to Europe since coming to America in the fall of 1881. Leaving Washington May 1, accompanied by Mrs. Stejneger, he attended the International Fisheries Exposition at Bergen, and also the Fourth International Zoological Congress which convened in Cambridge, England, August 23. At this Congress he was elected to the International Committee on Zoological Nomenclature on which he remained as an active and influential member until his death. He was also a member of the Permanent Committee that handled the affairs of these Congresses. His travels following the Congress took him to various museums in northern Europe, where he examined types and other specimens and studied methods in museum procedure.

For its displays in connection with the Pan-American Exposition, held in Buffalo in 1901, the Smithsonian required collections of zoological materials from the West Indies, and to supply part of this material Dr. Stejneger, accompanied by Charles W. Richmond, Assistant Curator in the division of birds, travelled in Puerto Rico, then an area not well known. They reached the island February 12, 1900, and on February 20 continued from San Juan to Mameyes where, according to Richmond's diary, "the whole town turned out to see us." The following morning they moved to the finca of Señor Agostini located in the foothills of the mountain El Yunque. From this base they estab-

lished a camp for five days at about 3000 feet elevation in the rain forest that covers the upper slopes of this mountain. Here they worked in constant rain and fog, securing many fine specimens, and making many interesting observations. March 3 they returned to the lowlands to the town of Luquillo. Here both of the naturalists came down with malaria. Stejneger made a trip to San Juan for medicines, after which on March 9 the two moved on to Fajardo, but finally returned to San Juan. March 22, after an overnight trip by steamer, they arrived at Vieques Island where they collected until March 29, and then returned by steamer to San Juan. On March 31 they reached Arecibo, and Richmond notes that "I had good sleep, but Dr. S. was troubled with bedbugs and fleas," not an unusual complaint in those early days as I found from personal experience 12 years later. From here they went inland into the hills to Utuado, known from the early work of the naturalist Juan Gundlach, and then to a greater elevation at Adjuntas. They crossed the island by coach to Ponce and on April 17 returned by the same means of travel to San Juan, a long journey with an overnight stop in Cayey. April 19 they embarked for Santiago, Cuba, and New York City. The original plan had been to continue through the Lesser Antilles, but illness through which they lost nearly three weeks time made it desirable to return north. This, Stejneger's only venture in the tropics, resulted in his scholarly treatise on the Herpetology of Porto Rico, published in the Report of U. S. National Museum for 1902 (issued in 1904) which today remains a model of its kind.

In the summer of 1901 Stejneger represented the Smithsonian Institution and the U. S. National Museum at the Fifth International Zoological Congress in Berlin. The German Parliament, the Reichstagsgebäude, was placed at the disposal of the Congress for its sessions, and the 750 delegates were lavishly entertained in Berlin, and later in Hamburg. After the close of the Congress he worked at the zoological museums in Berlin, Hamburg, Dresden and other cities. In his report Stejneger speaks especially of the progress made toward the formulation of a complete code of nomenclature. In the series of such Con-

gresses that came in succeeding years Stejneger took a prominent part, both because of his breadth of scientific knowledge and because of his ability as a linguist. Through these contacts he thus maintained personal relations with workers of the whole of Europe to a degree that has been equalled by comparatively few vertebrate zoologists of the United States.

In 1903 the annual report of the National Museum notes that Stejneger served as Acting Head Curator of the Department of Biology for August and part of October, indication of the position later in store for him. In 1904, with Gerrit S. Miller, jr., then Assistant Curator of Mammals, and C. W. Stiles, Custodian of the Helminthological Collections, Stejneger represented the Institution at the Sixth International Congress of Zoology held in Bern, Switzerland, from August 14 to 19. In addition to the examination of specimens in various museums Stejneger and Miller made collections of mammals, birds, reptiles, amphibians, crustaceans, insects, mollusks, and plants at various localities in the Swiss, French and Italian Alps, covering the western section of that range of mountains. Their studies were concerned principally with data on the life zones for comparison with the similar detailed studies that had been carried forward with such success in North America. Their work began near Genoa and then continued into the mountains, in part together, and in part separated, that they might cover more ground. In this way they secured photographs and specimens—mammals, birds, reptiles and plants—in the region surrounding Aix-les-Bains, Geneva, St. Cergue, Chamonix, Zermatt, Grindelwald, Vitznau, and Göschenen.

The following year Stejneger was again abroad as representative of Smithsonian interests at the Fourth International Ornithological Congress, held in London from June 12 to 17, 1905. In recognition of his extensive work in ornithology he remained a member of the Permanent Committee of One Hundred of the International Ornithological Congresses until his death. Following this Congress he also attended the convention of the International Catalogue of Scientific Literature, held in London from July 25 to 30. He remained abroad for over

three months on a detail that gave him opportunity to study material in connection with his investigations on the reptiles and amphibians of Japan and the West Indies in the museums of Bergen, Kristiania, Stockholm and Copenhagen, and also in Cologne and Altona. While in Denmark he made small collections of mammals, and some other biological material especially wanted for the museum, from the island of Laaland, an important type locality.

Having undertaken studies looking toward a revision of the salamanders he found it was necessary in the summer of 1906 to make some investigations in the field. For this purpose he left Washington July 16 and located at the O'Connell farm, a mile north of Stribling Springs, Augusta County, Virginia, at an elevation of 1700 feet on the south slope of Lookout Mountain, remaining until August 28. The area was selected because of an abundance of springs and small streams, and because it lay in a geographical position that made available a considerable number of species. The work centered especially on observations of living salamanders and on the collection of larval forms.

The following year he was one of the delegates for the Seventh International Zoological Congress, held in Boston from August 19 to 23, and served as organizing secretary to the section of zoogeography. Also in this year there appeared one of his more important scientific contributions, the Herpetology of Japan and Adjacent Territory, a bulletin of the U. S. National Museum that covered 577 pages.

In the summer of 1910 the collections in the division of reptiles and batrachians were moved to the new museum building that had just been completed, so that Stejneger had the major task of the rearrangement of his entire collection in new quarters. Advantage was taken of the opportunity to verify many of the older records, important work that took away from time ordinarily available for other research. It was this necessity that prevented his attendance at the Eighth International Zoological Congress held in August 1910, at Graz, Austria.

On June 1, 1911 Dr. Stejneger was appointed Head Curator of the Department of Biology, a post left vacant by the advance-

ment of Dr. F. W. True, former incumbent, to Assistant Secretary of the Smithsonian Institution. Stejneger's broad knowledge in systematic biology and his extensive experience fitted him especially for this position, one that he occupied with distinction until his death. At the same time he continued in charge of his former division, though necessarily his scientific investigations were curtailed by his enlarged administrative responsibilities. In the summer of this same year he attended the Centenary of the Royal Frederick University at Kristiania, and the 500-year celebration of the University of St. Andrews in Scotland.

In 1913 he was abroad again as one of the representatives of the Institution at the Ninth International Congress of Zoology at Monaco from March 25 to 30. Following the Congress, through a grant from the Smithsonian Institution, he made further life zone studies in the Alps, extending the area covered in his previous work with Miller in 1904. Beginning April 20 he devoted a month to the region between Switzerland and the head of the Adriatic Sea, where his observations covered especially the Val Sugana and the Plateau of the Sette Comuni, a region politically partly in Italy and partly in Austrian Tirol. Further travels took him into the Etsch Valley, and that of the tributary stream, the Eisak, as far as the Brenner Pass. He was able to trace the zones here in considerable detail, and obtained data of much value for correlation with those outlined for North America.

In his position as head of the department of biology his time was more and more absorbed by administrative work, not only in the direction of his department, the largest in the National Museum, but also in matters that concerned the museum as a whole. For years he served as chairman of a committee that considered all of the manuscripts offered for publication to the Smithsonian, and he also had many calls on his time for help from students interested in herpetology. His own published contributions continued, though reduced considerably in volume, and he worked regularly on various scientific problems, in these

later years mainly in herpetology, with only occasional contributions relating to birds.

In the summer of 1922 on behalf of the United States Department of Commerce Dr. Stejneger, at the age of 70 years, made another examination of the northern fur-seal rookeries to determine the conditions that had developed since the last international agreements had been reached in 1911. With Capt. C. E. Lindquist as assistant he left Unalaska June 20 on the Coast Guard cutter *Mojave,* in company with a party under Assistant Secretary of Commerce C. H. Houston that was to study Alaskan fisheries and other matters. The ship made frequent stops in this work on the way north, so that the party did not reach Unalaska until July 10. Stejneger and Lindquist transferred at once to the Coast Guard cutter *Algonquin* and continued to the Pribilof Islands where the increase in the number of seals appeared remarkable, largely because of the elimination of pelagic sealing. The vessel returned to Unalaska to refuel and then left for the Commander Islands, arriving at Nikolski village on Bering Island July 24. Here Stejneger and Lindquist remained while the ship returned to Unalaska. The seal herds that had contained 30,000 animals in 1897 now were found to number only 2,000, restricted to the North Rookery. In fact there were so few remaining that regular killing had been abandoned. In fog and rain the men worked here until the *Mojave* arrived, and then proceeded via Petropaulski to the Japanese controlled Robben Island. The seal herd here, under methods adapted from those in use on the Pribilofs, had increased greatly, indicating the value of the protection given. The *Mojave* then continued to Hakodate and Yokohama, where further information on seals was obtained. There was also opportunity to visit the Imperial Fisheries Bureau in Tokyo, and the Biological Station at Misaki, before sailing for Seattle on the *President Jefferson* on September 2.

In 1927 Stejneger was abroad as official delegate for the United States and for the Smithsonian at the Tenth International Zoological Congress, convened in Budapest in early September. At the same time he visited a number of European museums to

examine type specimens, and also to recover various collections of biological material lent to European scientists during the period prior to World War I. In 1930 he made a similar journey to the Eleventh International Zoological Congress meeting at Padua, Italy at the end of August; and in early September he likewise visited a number of museums and scientific institutions, especially those in Berlin, where he spent some time in consulting publications not available in Washington.

On July 11, 1932, while crossing the street he was knocked unconscious by an automobile opposite the Constitution Avenue entrance of the Natural History Building. Fortunately his injuries were not severe but it was prudent for him to remain quiet for a time. I recall very clearly that to force him to take proper care of himself I had to order him officially to remain at home for two weeks as he was insistent that he should return to his office three days after the accident.

In 1935 Dr. Stejneger attended the International Entomological Congress in Madrid from September 7 to 12. Following this he was chairman of the American delegation at the Twelfth International Zoological Congress in Lisbon from September 14 to 21. His last journey to Europe came in the late summer of 1939 when he crossed to Norway to attend the 65th Anniversary of his graduation at the University of Oslo (known earlier as the University of Kristiania) an experience that can come to comparatively few men. He was also to represent the Smithsonian Institution at the two hundredth anniversary of the Royal Swedish Academy of Sciences and went to Stockholm for the purpose. Though the full plans for the general celebration had been abandoned because of the sudden outbreak of war he was received at a special ceremony where he presented the congratulations of the Institution.

Though Stejneger attained the normal retirement age of 70 on October 30, 1921, because of his excellent physical and mental condition and outstanding abilities he was continued in service. In 1932, under a new law that made retirement mandatory except under highly exceptional circumstances, he was one

of a very few who on June 30 of that year were exempted indefinitely from compulsory retirement by an Executive Order signed by President Hoover. This action permitted his continuation in the service of the Smithsonian Institution until his death.

On his eightieth birthday, on October 30, 1931, the American Society of Ichthyologists and Herpetologists devoted a number of the society's journal Copeia to Dr. Stejneger, publishing a biography by Dr. A. K. Fisher, and a series of papers by his scientific associates. In the same year the society elected him Honorary President for life. On the occasion of his eighty-fifth birthday in 1936, eighty-seven of his friends and associates joined in purchasing a portrait of him for presentation to the National Museum, a most excellent likeness painted from life by the artist Bjorn Egeli. To celebrate his eighty-sixth birthday Stejneger's friends and associates gave him a dinner at the Cosmos Club on October 30, 1937, with Dr. Charles Greeley Abbot, Secretary of the Smithsonian Institution presiding. His Excellency Wilhelm Munthe Morgenstierne, Norwegian Minister to the United States, Alexander Wetmore (through Herbert Friedmann), Albert Hazen Wright, William Mann, Charles Wardell Stiles and Albert Kenrick Fisher presented papers in his honor. The friends gathered on this evening later received from Stejneger a card bearing an excellent reproduction of the Egeli portrait mentioned above, beneath which was the simple inscription "Ex corde pleno".

Though he had been failing in strength appreciably over a period of two years his final illness was of short duration. His death came at 3 p.m. on Sunday, February 28, 1943.

Among Stejneger's many publications listed in the accompanying bibliography, other than those that have been described in previous pages there must be mention of the Check-list of North American Amphibians and Reptiles in which he collaborated with his firm and devoted friend Dr. Thomas Barbour, of the Museum of Comparative Zoölogy of Harvard University. This was first published by the Harvard University Press, December 12, 1917 as a volume of 125 pages. The second edition in

November, 1923 was enlarged to 171 pages, the third in June, 1933 to 185 pages, and the fourth in June, 1939 to 207 pages. The fifth edition, which covered 260 pages was in press at the time of his death, appearing finally in July, 1943 in the bulletin series of the Museum of Comparative Zoölogy. Under the authorship of these two recognized leaders the work has provided a standard for forms, names and distribution in the two groups of animals that it covers.

Another work that deserves special notice is a biography entitled Georg Wilhelm Steller, the pioneer of Alaskan natural history, issued by the Harvard University Press in August, 1936, as a book of 623 pages with 29 plates and other illustrations. When Stejneger went first to Bering Island in 1882 he had with him as one guide for his work in the field a handwritten copy of Steller's Beschreibung der Bering-insel, based on Steller's experiences when he wintered there in 1741-1742. Stejneger's experience in this first hand check on Steller's work so aroused his admiration for the pioneer naturalist in this desolate region that for fifty years he accumulated biographical data on him as opportunity offered. He searched in European archives, in church and family records, examined labels on scientific specimens, and studied a variety of other papers until he had accumulated the vast amount of data that he presented in this volume, where it forms not only an intriguing biography of a colorful character but also an historical document that will remain a standard of its kind. One of the highlights for the author in the assembling of this data came on the evening of June 29, 1922, when on his way north on his last visit to the Commander Islands he had opportunity to land for a few hours on Kayak Island, on the beach where Steller had first come ashore in 1741, and to collect a few specimens of the plants that Steller had described there. Stejneger's only regret with regard to this book was that the manuscript grew to such length that he could not hope to publish many of the photostats and other source materials that he had accumulated with such care.

Dr. Stejneger's scientific honors included the decorations of the Knight First Class of the Royal Norwegian Order of St.

Olav, given October 13, 1906, and Commander of the same Order in 1939. He was also a Life Member of the Bergen Museum. It is interesting to note that in these recognitions from the country of his birth his family name was spelled in the original form, i.e., Steineger. His election to the National Academy of Sciences came in 1923. In that same year he received the Walker Grand Prize of One Thousand Dollars of the Boston Society of Natural History "for his distinguished contributions to the science of herpetology." In 1937 he was made a member of the District of Columbia Chapter of the Society of Sigma Xi.

His memberships in scientific societies were many, including the folowing: American Ornithologists' Union (Fellow Emeritus), American Association for the Advancement of Science (Fellow), Zoological Society of London (Corresponding Member), Ornithologische Gesellschaft in Bayern (Corresponding Member), Academy of Natural Sciences of Philadelphia (Corresponding Member); Biological Society of Washington (President 1907-1908), American Society of Ichthyologists and Herpetologists (President 1919, Honorary President 1931 for life), Washington Academy of Sciences, Association of American Geographers, California Academy of Sciences (Honorary Member), British Ornithologists' Union (Honorary Member), American Society of Mammalogists (Charter Member, later Honorary Member), Deutsche Ornithologische Gesellschaft (Honorary Member), Oslo Academy of Science (Foreign Member), Peiping Natural History Society (Corresponding Member), Baird Ornithological Club (President, 1925) and the Washington Biologists' Field Club (Honorary Member).

Through his field activities, and his wide association with other scientists, it was natural that a variety of forms of life have been named in his honor. The list, segregated by groups, is as follows:

Mammals:

Mesoplodon stejnegeri True
Phoca stejnegeri Allen

Citellus stejnegeri Allen
Lutra stejnegeri Goldman

Birds:

Oidemia stejnegeri Ridgway
Hypsipetes amaurotis stejnegeri Hartert
Pratincola rubicola stejnegeri Parrot
Anthus stejnegeri Ridgway
Parus stejnegeri Bangs
Zosterops stejnegeri Seebohm
Chlorodrepania stejnegeri Wilson
Spindalis zena stejnegeri Cory
Chrysomitris stejnegeri Sharpe
Janthoenas janthina stejnegeri Kuroda

Amphibians:

Ambystoma stejnegeri Ruthven
Hymnobius stejnegeri Dunn
Borborocoetes stejnegeri Noble
Bufo stejnegeri Schmidt

Reptiles:

Anolis stejnegeri Barbour
Cnemidophorus stejnegeri Van Denburgh
Uta stansburiana stejnegeri Schmidt
Cyclura stejnegeri Barbour and Noble
Sphaerodactylus stejnegeri Cochran
Eumeces stejnegeri Taylor
Plica stejnegeri Burt and Burt
Crotalus stejnegeri Dunn
Typhlops stejnegeri Loveridge
Zamenis stejnegerianus Cope
Pseudemys stejnegeri Schmidt

Fishes:

Steinegeria rubescens Jordan and Evermann
Stelgistrum stejnegeri Jordan and Gilbert

Scaphognathus stejnegeri H. M. Smith
Sikukia stejnegeri H. M. Smith

Mollusks:

Cerithiopsis stejnegeri Dall
Volutopsius callorhinus stejnegeri Dall

Myriapoda:

Lithobius stejnegeri Bollman

Coleoptera:

Tachyporus stejnegeri Blackwelder

Medusae:

Haliclystus stejnegeri Kishinouye

Plants:

Alopecurus stejnegeri Vasey

Stejneger's own descriptions of forms new to science, mainly of birds, reptiles and amphibians number several hundred. The list of these may be ascertained by any interested persons from the papers in the appended bibliography.

In person Leonhard Stejneger was spare and slender, in movement active and energetic. In 1897 in San Francisco, when on his way to the Commander Islands, he recorded in his diary that he weighed 146 pounds, but in later years he fell below this mark. Though never seeming strong or robust he retained his physical powers in his advanced age in a truly remarkable manner. He was heavily bearded, both hair and beard becoming gray but not completely white. His home, in Washington, was a center for friendly hospitality among his fellow workers and associates, including many foreign scientists and visitors from Europe, and at times from other widely scattered places throughout the world. All will recall the gracious courtesy of their host. His more intimate friends will remember also his pleasure and his active skill in dancing on evenings when there was

music, a delight that finally, on physician's orders, he had to forego at the age of 85.

Dr. Stejneger bore himself always with dignity, but at the same time was friendly and not at all aloof. The younger workers in zoology who had contact with him found him almost universally kindly and helpful, and few men of his accomplishments have devoted as much time to assisting and advising others, always freely, and without personal bias or selfishness. At the same time he was definite and positive in his ideas, which he advanced in direct and forceful speech and without hesitation. With the knowledge gained through his long experience he coupled a clarity of thought that gave his opinions weight and authority, and those with whom his ideas were at complete variance were inclined to feel that his expression of them was presented without personal animus since while holding to his own views at the same time he had tolerance for those of others. In his scientific researches he was progressive, sound and conservative so that he became one of the important international figures not alone in ornithology and herpetology, in which he worked especially, but in various other branches of systematic zoology as well. His many publications have been standards on which much modern progress in these fields has been based, their influence reaching throughout their part of the scientific world.

Not until he had gone did the younger members of the Smithsonian staff who were closely associated with him realize fully how frequently they consulted him, and the weight that his opinions carried. And this extended into wide fields of knowledge beyond those where he had prosecuted active research.

The accomplishments of workers in the fields of taxonomic research and of geographic distribution are measured not in terms of single discoveries but more as a summation of the total of their results. With a bibliography of serious publication of research beginning in 1871, and extending to the time of his death, we see that the productive period of Stejneger's scientific contributions of more than 72 years was longer than the usual life span of the average scientist. His influence, therefore, has been correspondingly great, not only through the considerable volume

of his investigations but also through the quality of his researches in terms of lasting value from their careful and thoughtful presentation. His papers on the whole remain as basic works of reference in the later researches of today. An eminent Danish writer remarked to me recently that he still used Stejneger's little manual of the birds of Norway, published in 1873, as his handbook for field identification.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

Amer. Geogr. Soc. Research Ser. = American Geographical Society Research Series
Amer. Geogr. Soc. Special Publ. = American Geographical Society Special Publication
Amer. Nat. = American Naturalist
Ann. Rep. U. S. Nat. Mus. = Annual Report United States National Museum
Arch. Math. Naturv. = Archiv för Mathematik og Naturvidenskab. Oslo
Bergens Mus. Årb. = Bergens Museum Årbok
Bull. Amer. Geogr. Soc. = Bulletin, American Geographical Society
Bull. Bur. Fish. = Bulletin, Bureau of Fisheries
Bull. Mus. Comp. Zoöl. = Bulletin, Museum of Comparative Zoölogy
Bull. U. S. Fish. Comm. = Bulletin, United States Fish Commission
Deutsche Geogr. Blätt. = Deutsche Geographische Blätter
Geogr. Rev. = Geographical Review
Journ. Bombay Nat. Hist. Soc. = Journal, Bombay Natural History Society
Journ. für Orn. = Journal für Ornithologie
Journ. Sci. Coll. Imp. Univ., Tokyo = Journal, Science College, Imperial University, Tokyo
Journ. Washington Acad. Sci. = Journal, Washington Academy of Sciences
Nat. Acad. Sci. Biogr. Mem. = National Academy of Sciences, Biographical Memoirs
Norsk Veterin. Tidsskr. = Norsk Veterinær-Tidsskrift
Norske Geogr. Selsk. Aarbog = Norske Geografiske Selskabs Aarbog
North Amer. Fauna = North American Fauna, Bureau of Biological Survey, United States Department of Agriculture
Nyt Mag. Naturv. = Nyt Magazin for Naturvidenskapens. Oslo
Occ. Pap. Boston Soc. Nat. Hist. = Occasional Papers, Boston Society of Natural History
Orn. Centralbl. = Ornithologisches Centralblatt
Pop. Sci. = Popular Science
Proc. Acad. Nat. Sci. Phila. = Proceedings, Academy of Natural Sciences, Philadelphia
Proc. Biol. Soc. Washington = Proceedings, Biological Society of Washington
Proc. Boston Soc. Nat. Hist. = Proceedings, Boston Society of Natural History
Proc. U. S. Nat. Mus. = Proceedings, United States National Museum
Smithsonian Misc. Coll. = Smithsonian Miscellaneous Collections

U. S. Nat. Mus. Bull. = United States National Museum Bulletin
West Amer. Scient. = West American Scientist
Zeitschr. Ges. Orn. = Zeitschrift für die gesammte Ornithologie. Budapest

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Douglas Johnson

NATIONAL ACADEMY OF SCIENCES

OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XXIV—FIFTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

DOUGLAS WILSON JOHNSON

1878-1944

BY

WALTER H. BUCHER

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1946

DOUGLAS WILSON JOHNSON[1]
1878-1944

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Family Background

Douglas Wilson Johnson was born into a thoroughly American family and environment not far from what was at the time the population center of the United States. His branch of the Johnson family had come to the New World not later than the beginning of the 18th Century. His great-great-grandfather, Abraham Johnson, had moved in 1750 from Essex County, New Jersey, to Patterson Creek on the Potomac, below Cumberland, Maryland, on the Virginia side (now West Virginia). Thence his grandfather, William Johnson, crossed the Alleghenies and took a homestead in the Ohio Valley at a place bearing the telling name "Long Reach," some thirty miles above Parkersburg, Virginia (now West Virginia). He cleared the land with the help of slaves, developed a farm, and bred cattle. He married twice, and reared thirteen sons and two daughters (four of nineteen children born to him died in infancy). He was a man of rigid standards of right and justice, and he was intensely religious. This made him a stern disciplinarian on the one hand, and on the other made him free his slaves and pay their way back to Liberia when he became convinced that slavery was wrong. He maintained a school at his homestead for his own children and those of the neighbors who wished to attend. Getting a good teacher was always more important than getting a good price for his cattle. The financial support of the country church devolved mainly on him, and often the preacher was also the teacher in his school.

The five sons from his first marriage and two from the second

[1] The materials presented in these pages were derived largely from the biographical record which Professor Johnson had filed with the National Academy of Sciences and from records made available by the Department of Geology of Columbia University. The writer is deeply indebted to Professor Johnson's sister, Mrs. Edward H. Reisner, who gave the writer a vivid picture of the family background and her brother's formative years. To Dr. Frank J. Wright, the writer owes special thanks for making available to him in manuscript form his comprehensive memorial to Douglas Johnson which has now appeared in print (Proceedings Volume of the Geol. Soc. of America, Annual Report for 1944, May 1945, pp. 223-239). An intimate portrait of Douglas Johnson from the subtle pen of Professor Armin K. Lobeck, helped round out the picture of the man whom the writer had known well only in his last years. (Annals Assoc. American Geogr., Vol. 34, 1944, pp. 216-22.)

became farmers; of the others, four turned to law, one ran a village store, and one entered the ministry. The latter, Thomas Carskadon Johnson, was for many years the leading Baptist clergyman in West Virginia, pastor of the First Baptist Church at Charleston, W. Va. The most prominent of those who made law their profession were Okey Johnson, Chief Justice of the West Virginia Supreme Court and later dean of the Law School of the West Virginia State University, and David Johnson of Parkersburg, West Virginia. The youngest of the sons who turned to farming, Daniel Dye Johnson had also studied law. He did much to foster scientific agriculture in his state and eventually helped to establish a department of agriculture at the University of West Virginia.

Douglas Johnson's father, Isaac Hollenback Johnson, was born in 1838, at Long Reach, Tyler Co., Virginia (now West Virginia). He worked on his father's farm as a boy, graduated from Marietta College, kept a country store for a few years, and then turned to law. But by that time an early interest in prohibition had risen in him to the flame of the reformer's zeal. He soon gave up his law practice to become one of the pioneers of prohibition, that most American of social experiments. His efforts led to the introduction of the first local option in West Virginia. He threw his whole energy and every cent he owned into the stubborn fight for the cause and especially into the maintenance of "The Freeman," the paper of which he was editor and part owner. Incessant financial and other worries undermined his strength. He went to South Dakota to regain his health, contracted pneumonia and died in 1891, at the age of 53.

Throughout this harrowing struggle, Isaac Johnson had the whole-hearted and able support of his gifted wife, Jane Amanda Wilson. Her maternal grandfather, Judge Israel B. Grant of Calloway County, Missouri, had been county judge and for a time member of the State Legislature. She had been educated in the Ladies' Seminary at Liberty, Mo., and had early shown talent for speaking and writing. Married at the age of 21 she shared the development of her husband into a crusader. With a

facile pen she contributed liberally to his paper. She worked untiringly as speaker and organizer for the Women's Christian Temperance Union. As chairman of that organization in the state of West Virginia, repeatedly re-elected, she showed great executive and parliamentarian skill. She was an ardent advocate of women's suffrage and took active part in all political affairs and church work.

She was highly intellectual, reading incessantly to keep informed on the problems of the day. She was, for instance, one of the first persons in her community to own a copy of Darwin's "Origin of Species," passing it about to her friends.

Jane Amanda Wilson Johnson bore six children of whom two died in early childhood. When her husband died, Sam was 23, Douglas 12, Elizabeth 9, and Ellen 7 years old. With the aid of her oldest son, Mrs. Johnson continued her life's work, ruling her children with a stern hand. At last her health declined. She had been an invalid from asthma for two decades, but tuberculosis set in at the age of 55, and she died in New Mexico, where she had sought recovery.

Formative Years

Into this home, rich with intellectual and reformative zeal, with writing, printing and speech making, and strained by incessant political and financial struggles, Douglas Johnson was born on November 30, 1878, at Parkersburg, W. Va. He was a frail, timid, shy child, often ill. He was an easy target for bullies when he entered school, until his older brother Sam spurred him to disregard mother's advice and fight back. He did so, though paralyzed with fear, and won victory and self-respect.

Initiative, creative urge, ambition, and imaginative power showed early in his life. Even as a young boy, when playing with his sisters and friends, Douglas took charge of planning and directing games. He delighted in creating occasions at which he would function as master of ceremonies. At the age of ten he would write out elaborate speeches and detailed pro-

grams for such occasions.[2] When twelve years old, he announced his intention to write a New Testament "in language that people can understand."

In school at Parkersburg, W. Va., he won the Prager prize three years in succession, the largest number of times its terms permitted it to be awarded to the same pupil.

As a boy, Johnson never took any interest in making collections of any kind. In later years, he did not even remember the large collection of Indian relics which his older brother Sam had made and later had given to the University of Arizona. Douglas' interest centered in declamation, oratorical contests, literary societies. He showed no scientific inclinations.

His mother had early realized that Douglas would have to make his living with his brain, and would, of course, go to college.[3]

In 1896, eighteen years old, Douglas went to Granville, Ohio, to enter Denison University as a student. During the two years he spent in that charming little college town he boarded in the home of a Baptist preacher, the Rev. Frank Adkins, a friend and college mate of his father's. Soon he found himself in love with Alice, the daughter of his host, a highly intelligent and sensitive girl who faced a life of blindness, her eyesight failing rapidly. With characteristic idealism and determination, Douglas wooed her and became engaged to her, a year after his arrival at Granville, undeterred by her affliction and by danger signs in his own health.

Fearing tuberculosis, Douglas transferred to the territorial University of New Mexico, at Albuquerque. Four years earlier, Professor Clarence Luther Herrick, one of the most beloved of

[2] For example; For the formal installation of a new toy elephant in place of one that had gone to pieces, he wrote out a program entitled, "Coronation of Prince Jumbo, heir apparent to the throne of King Jumbo and Princess Georgiana." As coronation hymn, he used a Sunday School song: "My father is rich in houses and lands, he holdeth the wealth of the world in his hands."

[3] Young Douglas earned money where he could. Between his fourteenth and seventeenth year he helped his brother Sam in his job-printing office in the evenings. [illegible] ling "Doctor Chase's Receipt [illegible] in country districts, and teaching in a country school (for five months). After he had thus shown his mettle, his mother's cousin paid his college expenses as "loans." Secretly he tore up the notes.

Denison's teachers, had gone to New Mexico and had since become president of the University. He took a special interest in the newly arrived Denison student and gave him the opportunity of becoming his assistant in geological field work during the summer months.

This association became crucial in Douglas' life. He had accepted the work primarily because it gave him the opportunity of living outdoors. But through it he came into close contact with a man who was famous for "his rare power of influencing young men." Professor Herrick set before this impressionable, idealistic, and ambitious young man an eloquent example of selfless devotion to natural science, a philosophic approach to scientific problems, conspicuous mental independence, and reckless industry and ceaseless drive. He turned the young man who seemed headed for a public and literary career, into a man of science and set for him the pattern that dominated his whole life. Fifty years later Johnson spoke with gratitude of the impact of this great teacher on his development.

In 1901, Douglas graduated from the University of New Mexico with the B.S. degree after having won the Finical gold medal in an oratorical contest. In order to finance graduate work, he taught in the high school of Albuquerque for one year and then went east for graduate work at Columbia University where he received the Ph.D. degree in 1903.

The Geomorphologist

Douglas Johnson began his academic career by accepting an instructorship in geology at the Massachusetts Institute of Technology, while continuing his graduate studies at Harvard University. In the same year, 1903, he married Alice Adkins.

In the preceding year, William Morris Davis' "Physical Geography" had appeared and for the first time made generally available in simple form the imaginative terminology which his genius had created, a terminology based on the innate logic that orders the multitude of topographic forms into ontogenetic series as inevitable as the growth stages of an organism. The eloquence with word and pen of this great master, who listed his courses

under the title of "Physical Geography," drew disciples primarily from the ranks of geologists. Douglas Johnson was one of them. He quickly made himself a master of Davis' method of deductive reasoning that draws forth the very last consequence from every working hypothesis. He also became thoroughly imbued with Davis' unrelenting zeal for clarity of exposition and punctilious care in the choice of words. Johnson may well be said to have been the most distinguished of the many disciples of William Morris Davis.

For years his connection with Davis was intimate, first as instructor and assistant professor in geology at the Massachusetts Institute of Technology (1903-1907) and then as assistant professor of geology at Harvard (1907-1912). During that time he edited Davis' "Geographical Essays," a book of 776 pages, published in 1909, thereby rendering a major service to geography and geology as well as to the master.

But, brilliant disciple that he was, he was too strong a man to remain a follower. While still at Harvard, he struck out on work in which he combined detailed observation with broad analysis along lines quite his own.

In 1911, a short note in *Science* revealed the first major task to which he turned his critical mind: the question of recent subsidence of the Atlantic coast. This had been calculated as one foot per century for the Massachusetts coast and as high as twice that amount for the New Jersey coast. Douglas Johnson challenged the validity of the criteria on which these figures were based. This led on the one hand to a critical study of all shore processes, and on the other to detailed studies of tidal gauge and precise leveling data.

The first line of study, in 1911, took the form of the Shaler Memorial Expedition, an elaborate and exhaustive study of the whole shoreline of Eastern North America from Prince Edward Island to the Florida keys. For comparison, Johnson visited the shores of England, Scotland, Sweden, Holland, and Germany. Throughout these travels he studied local details as guides to, and illustrations of, fundamental principles, never as ends in themselves. Out of these investigations grew two

books of outstanding merit: "Shore Processes and Shoreline Development" (1919) and "The New England-Acadian Shoreline" (1925). The latter was awarded the A. Cressy Morrison prize by the New York Academy of Sciences.

The second line of study led, in 1923, to the formation of a Committee on Shoreline Investigations by the National Research Council, with Johnson as chairman. Two projects were carried through by this committee with signal success.

(1) Johnson's concept of local variations in mean sea level was subjected to a rigid test, with the cooperation of the U. S. Coast and Geodetic Survey, the Department of Docks and the Department of Plant and Structure of New York City. The results were published in 1929 by Johnson as Bulletin 70 of the National Research Council: "Studies of Mean Sea Level."

(2) The second project, which was to stimulate interest in the systematic collection of data concerning changes in the beaches of the Atlantic seaboard, brought about correspondence and personal contacts with officials, especially in New Jersey, which eventually led to the formation of the American Shore and Beach Preservation Association which, in turn, was a factor in the formation of the Beach Erosion Board in the Corps of Engineers of the U. S. Army.

Douglas Johnson's intimate familiarity with the Atlantic coast of North America and that of Europe, led to critical studies on the correlation of marine terraces. He extended his personal observations to parts of Algeria and South Africa, Western and Southeastern Australia, New Zealand, Japan, and Hawaii. He saw and discussed classical localities under the guidance of those who knew them authoritatively. As president of the Commission for the Study of Pleistocene and Pliocene Terraces of the International Geographical Union (1934-38), he was in touch with terrace studies carried on in different parts of the world. He traveled again along the Atlantic and Gulf coasts to determine the causes of the wide divergence in the correlations of terraces in these regions. With characteristic patience and thoroughness he was gathering the materials for a major work on the correlation of terraces, one of the most intricate questions

in geomorphology and historical geology. But that work never took form. The presidential address of 1942, which was never presented and was published posthumously, deals only with basic questions of technique and analysis.

The development of stream systems, especially those of the Atlantic slope of the United States, constitutes the second major field of research with which Douglas Johnson's name has become identified. In his book on "Stream Sculpture on the Atlantic Slope" (1931), he challenged Davis' classical interpretation with arguments that are now widely recognized and may well prove permanently valid in the face of steadily growing factual knowledge and theoretical understanding.

Perhaps his most important contribution to geomorphological theory is the concept of rock fans and its application to the interpretation of pediments. His classical paper on rock fans in arid regions was presented at the Tulsa meeting of the Geological Society of America and was published in 1932. In the summer of the following year, Professor Johnson gave an *ex tempore* exposition of his views on the origin of rock fans and pediments before an international party of geologists that was touring the West under the auspices of the 16th International Geological Congress. For clarity of reasoning and beauty and simplicity of speech this address stands unrivaled in the writer's memory.

In the last years of his life, Douglas Johnson turned his powers of analysis on two features of the Atlantic slope that had come to the forefront of interest among geologists and geomorphologists only relatively recently: The submarine canyons, and the strange, widely scattered so-called "meteorite scars" of the Carolina coast, the "Carolina bays." The book on "The Origin of Submarine Canyons" appeared in 1939, that on "The Origin of the Carolina Bays" in 1942. In a review published in the *Geological Magazine* (London), C. A. Cotton characterizes the latter, the last of Johnson's larger works as "a model of scientific method." "It is equally a model of literary style," he wrote. "The book is the work of a teacher and a scholar and, above all, of a gentleman."

The subjects so far mentioned represent the central thread of major investigations in geomorphology, his central field of research. Around it he wove the strands of shorter studies that grew, directly or indirectly, out of field work with Herrick's survey of New Mexico (1889-1901) and with the United States Geological Survey (1901, 1903, 1904, 1905), and later out of independent field studies and observations, and, especially, out of his teaching. They range from the physical history of the Grand Canyon district, the hanging valleys of the Yosemite, nature and origin of fjords, mussel distribution as evidence of drainage changes, to map projections and map drawing in schools.

With advancing years, Professor Johnson turned his attention more frequently to the formulation of the principles involved in scientific reasoning. The paper he read in 1933 as retiring vice-president and chairman of Section E of the American Association for the Advancement of Science, on the "rôle of analysis in scientific investigation" (1933)[4] stands out as a companion to G. K. Gilbert's famous address on "The inculcation of scientific method by example" (1886), T. C. Chamberlin's "The method of multiple working hypotheses" (1897), and W. M. Davis' on "The science of geographical investigation" (1911).

In this paper, speaking of inductive and deductive reasoning he remarks: "The wise investigator will use one to supplement the other; for he secures a great advantage if he first employs inductive reasoning to derive from observed facts certain general conclusions, then reverses the process and, using the conclusions as working hypotheses, deduces their reasonable consequences, checking these last against observed facts as the best proof of the correctness of his reasoning." In this skillful interplay of induction and deduction he was a master, tireless in the search for data, rigorous in their analysis, fertile in the invention of working hypotheses, stern and insistent in his logical conclusions.

[4] A treatment of the same and related subjects, planned on a larger scale, was begun in the "Journal of Geomorphology" under the title "Studies in scientific method," but remained incomplete, in fact, never reached the major topic at all.

The Teacher

The qualities just mentioned, combined with a wholehearted enthusiasm for his scientific work and an inborn desire to share it with others, made him an outstanding teacher who left an indelible impression on all who came under his influence. The distinguishing feature of his instruction was the emphasis on method in reasoning and on precision in the oral and written presentation of results. His graduate seminar in geomorphology was perhaps unique along these lines. It was devoted to reports by the students on their progress in research undertaken in connection with higher degrees. Each student was given fifteen minutes for one report and was held strictly to the time limit. He was expected to prepare what he had to say with the same care he would devote to the presentation of a paper before a learned society. The seminar was, in fact, designed deliberately to prepare the students for the delivery of scientific addresses and for public debate. As part of this training, each student had to prepare and post a typed abstract of his next report well ahead of the day of presentation. The form and adequacy of this abstract as well as the soundness of reasoning and method of presentation were then critically analyzed by the whole class, in addition to the subject matter itself. Professor Johnson reserved comment until the students had exhausted their criticism. Then he weighed the arguments, exposed errors, castigated loose thinking and lax speech, and bestowed praise where it was deserved. Though he encouraged free discussion, he maintained throughout a rigid formality that set the master apart from the disciples.

While he budgeted his time with utmost care, he gave of it freely to those who wrote theses under him. He spared no pains, scrutinizing every phrase, demanding revision after revision with the same unbending rigor that he applied to his own work. He did nothing casually and demanded the same of his students.

He organized field trips with the same scrupulous attention to all details. In the summers, he spent much time in the field

with students working on problems under him. In later years he took all such students in motor cars into the field as a group, giving all the benefit of the critical examination of the results achieved by each in his area. It was in the course of one of the last, if not the last, of these joint trips that the present writer first learned to know Professor Johnson as a teacher. It was an unforgettable experience, of which more will be said later.

Professor Johnson began his academic teaching career as instructor in the Department of Geology at the Massachusetts Institute of Technology in 1903. He was made Assistant Professor in 1905, and stayed until 1907, when he transferred to Harvard University, (where he had already begun to teach in 1906), with the rank of Assistant Professor. In 1912 he accepted the position of Associate Professor at the Department of Geology in Columbia University where he remained the rest of his life. In 1919 he was made Professor. From 1937 to 1944 he served as Executive Officer of the Department. In 1943 Columbia University bestowed on him the special honor of the Newberry Professorship.

As Executive Officer of the Department of Geology, and as chairman of the University's important Committee on Graduate Instruction (1938-42), he worked unceasingly for the promotion of creative scholarship at the highest academic level. The complete sincerity and balanced decisiveness of his judgments, his fine tact and world-wide experience made him a leader in the affairs of Columbia University, which he served with great loyalty and zeal.

In 1923-24, he extended his influence as a teacher to Europe. As exchange professor to France, he lectured at twelve universities, representing seven American universities (Columbia, Cornell, Harvard, Johns Hopkins, Pennsylvania, Yale, and the Massachusetts Institute of Technology). Out of this experience grew his volume entitled, "Paysages et Problèmes Géographiques de la terre Américaine," (1927) which introduced the French scientific public to the American method of geomorphological analysis.

War Geography, National and International Contacts

Creative work in his chosen field in pure science and the training of research men and teachers were the central purposes of Douglas Johnson's life. Yet he was early drawn into practical applications that brought him into close contact with world affairs and the men behind them.

From the beginning, Johnson followed the First World War in detail with the aid of large-scale maps, "anxious to discover how far modern military operations are still affected by the element of terrain." This led to several papers that were published in the Bulletin of the American Geographical Society and its successor, the Geographical Review. Some of these were reprinted in the Journal of the Military Service Institution. These, together with new materials, were put in book-form and published in 1917 under the title "Topography and Strategy in the War."

These researches led to a major's commission with the Intelligence Division of the U. S. Army. At the request of the Secretary of State, Douglas Johnson was directed to proceed to Europe "for the purpose of making special studies in military geography for the use of this Department (of State) in connection with the work being done at the direction of the President by Colonel E. M. House." With a view to future peace conferences and the inevitable rival claims, he was to secure firsthand information concerning the strategic and tactical value of landforms under the then modern conditions of warfare. With generous financial support from the American Geographical Society, which was then housing the staff of the "Inquiry" assembled under Colonel House's direction, and accompanied by one of his former students, Lieutenant S. H. Knight, professor of geology in the University of Wyoming, he visited the Belgian, British, French, American, Italian, and Balkan fronts. The war offices at London, Paris, Rome, and the various regional headquarters made available large-scale maps, relief models, and other facilities. In the field, a staff member familiar with the terrain and the military actions that had taken place on them was generally

assigned to accompany Johnson and to assist his studies. The results of these were published in 1921 by the American Geographical Society under the title "Battlefields of the World War." In a foreword, General Tasker H. Bliss gives rare praise to this volume:

"It is difficult to say whether this work of Professor Johnson has been written more for the benefit of the geographer and geologist, or of the military student, especially the student of the operations in the Great War, or of those who like to read charming descriptions in sweetly flowing English, of the physical landscapes in both their gay and gloomy moods. . . . To all of them it will be a classic, and to none more than to another."

At the conclusion of hostilities, Johnson was assigned to geographical investigations in preparation for the Peace Conference, and then went to Paris as Chief of the Division of Boundary Geography on the American delegation and as technical adviser to various commissions. Later he served as a member of several of the International Territorial Commissions, passing judgment on the strategic and tactical value of the terrain along certain proposed frontiers. Thus he gained a political perspective and made personal contacts that would have lured many another from the quiet academic goals of his youth. But in 1920 Johnson was back in his professor's chair, turning again to the problems of geomorphology with unimpaired enthusiasm.

He served repeatedly as consulting expert in boundary disputes and other legal cases that involved questions of a geomorphological nature. In 1926 he was called by the Canadian Government to act as consulting physiographer in the Labrador boundary disputes. Yet he never allowed commercial employment to become an end in itself. He wanted to be above all a student and a teacher.

In 1934, he accepted the difficult task of conducting an intensive investigation of the surveying and mapping activities of the U. S. Government for President Roosevelt's Science Advisory Board, as chairman of a committee on Mapping Services of the Federal Government. The resulting report of 163 pages "is

certainly the most exhaustive study ever made" of the tangled and overlapping surveying and mapping activities of 28 Federal Agencies. This included conclusions and recommendations "based on unanimous agreement of the committee." It is unfortunate that this important document is so little known and even a greater pity that it did not lead, at the time, to reorganizations which, if effected, would have been of incalculable value in the emergency of the second world war, apart from a saving of millions of dollars.

In the course of his career he became a member of many scientific societies and organizations. For his achievements he was awarded eight prizes and medals, and honorary degrees from six universities, three American and three European. Among the former was Columbia University which made him honorary Doctor of Science twenty-six years after it had granted the promising young student the degree of Doctor of Philosophy. Nine foreign learned societies made Johnson an honorary member and France and Yugoslavia decorated him. (For details see the Appendix.)

The Political Writer

Johnson's vigorous mind followed closely the shifting scenes of national and international contemporary history. He had no time to take an active part in local and national politics. But in the great crises of his time, unlike most other men of science,[5] he was not content to formulate the issues in his mind as sharply defined opinions, but translated them into convictions that led to action. He employed his forceful pen in the aid of the causes he considered just and aligned himself with organizations that worked for them.

During the first World War, he became one of the organizers and chairmen of the Executive Committee of The American Rights League which was founded in 1916 to secure American entry into the war. He wrote two booklets which reached very wide circulation and exerted a powerful influence in favor of

[5] See his editorial, "The Scientist as Citizen," Journal of Geomorphology, vol. 1, No. 1, Feb. 1938, pp. 62-63.

the Allied cause. The first one was originally not written for publication at all. It was a long and pointed reply to one of similar length received from a German colleague which like many others received after the outbreak of hostilities attempted to justify the German cause. In the hope that such an explicit statement of American opinion "might be some comfort" to his French colleagues, Johnson sent a copy of the letter to personal friends at the Sorbonne. It was translated and published, with his permission, in the *Revue de Paris* of September, 1916 and later issued in brochure form under the title "Lettre d'un Américain à un Allemand." The publicity department of the British Government then printed the English text as a pamphlet entitled "Plain words from America: a letter to a German professor" (1917). Extracts from the letter were translated into most of the languages of Europe. In America, Johnson's letter and the one to which it was written in reply, were published under the title "My German Correspondence" (1917).

An address, given before the annual convention of the Iowa Bankers Association at Des Moines two months after the United States had declared war on Germany, was printed under the title "The Peril of Prussianism." Its substance was reproduced in motion pictures for use in the American campaign of education.

These two booklets are documents that are worth reading today. Let three sentences be quoted from "My German Correspondence." They were addressed, not to a "Nazi" of our day, but to an intellectual leader of 1916:

"Your greatest enemy is not the Russian, nor the French, nor the British government. They might defeat you in war, but they never could take away your honor. . . . Your greatest enemy is the Government which stifles your individual development by making you the obedient tools of the "State," which smothers your free thought by a muzzled press under police control, which makes your learned men ridiculous in the eyes of the world by training them to blind, unthinking support of the Government and credulous belief in whatever falsehoods it chooses to impose upon you for military and political purposes, which hurls you into a disastrous war without your knowledge or consent, and which brings down upon you the contempt of the whole world

for crimes you would not yourselves commit, but which you must forsooth defend 'for the good of the State'."

The second occasion that called Johnson into political action was the change that took place in Washington after Franklin D. Roosevelt had assumed the leadership of a bewildered nation. As Johnson saw it, what happened was that "the letters spelling *New Leader* were shifted about, and the *New Dealer* began to play the leading role."[6]

Again Johnson weighed the issues and made up his mind. He set his face against the New Deal and the great man behind it. He joined the National Committee to Uphold Constitutional Government and put his pen to work. We find an article by him, directed against "bureaucratic control of industry and union tyranny over labor" in a pamphlet entitled "America's Future: one's right to work and how to implement it." In 1937, he wrote a seventy-page booklet: "The Assault on the Supreme Court—President's Purpose Exposed—Hidden Motives Bared." Over 300,000 copies were distributed by the Committee. Johnson felt deeply stirred and wrote with scathing effect.

One wonders what position this man with such a trenchant pen and such power of action might have achieved in public life had not that kindly quiet man in New Mexico taken him on long days in the desert and shown him the calm majesty of impartial scientific thought and the beauty of a life devoted to it.

One also admires the skill with which Johnson kept his life as a scientist separate from that of political action into which he was drawn from time to time. He pleaded "that scientists fight political battles with political weapons, and that they do all within their power to keep our academic halls and research laboratories sheltered from political storms, safe havens of intellectual sanity, calm judgment and free search for truth in a world gone mad."[7]

With the outbreak of the second World War we find him again taking part in public discussion. He wrote letters, some of which were sent to every daily paper in the United States

[6] Quoted from his foreword to the book by Samuel B. Pettengill, "Jefferson—the forgotten man."

[7] Quoted from his reply to a "Manifesto by a Physicist," 1939, p. 248.

on such subjects as "Repeal false Neutrality" and "An Answer to Colonel Lindbergh." The latter ends with words that have the old ring:

"To Lindbergh's pessimistic wail: 'France waited until it was too late. England waited until it was too late. We in America have waited until it is too late,' the American people will answer in true American fashion: 'Not on your life, Colonel! We haven't even begun to get ready yet'."

But his health was undermined. He had become a spectator rather than an actor, and soon the questions of the coming peace occupied him as much as those of the war's whirlwind. On that subject the writer knows of only one printed comment.

On September 24, 1941, Johnson gave the address at the opening of the fall session of Columbia University on "The next armistice—and after," devoted to a single thought. It is a fallacy to speak of "the ideal of justice against the practice of force." The opposite of "justice" is "injustice," not "force." The opposite of "force" is "weakness" or "impotence," not "justice" or "law." The obvious consequence he sums up in Pascal's words: "Justice without *force* is impotent. Force without *justice*—tyrannical. We must, therefore, combine *justice* with *force*."[8]

The Man

As a boy, Johnson saw early that great things were expected of him, and he resolved to make good. He also determined to strive positively for righteousness and to take a determined stand on all questions of personal and public conduct. With unrelenting drive and unerring purpose he followed this course throughout his years.

He ordered his life with rigid discipline and worked with steady concentration. He demanded the utmost of himself and expected no less of others. He did not drink or smoke and, at least in his younger years, did not hesitate when the occasion demanded to tell others how he felt in such matters. His was a stern sense of justice, inculcated no doubt by his mother's

[8] Quoted from "International Conciliation" No. 375, Dec. 1941, pp. 715-720.

Spartan discipline which he defended even as a boy. He was scrupulous in the fulfillment of even the smallest obligation. "To him it was a sin to fall down on an assignment," and he hated sin.

But decisiveness was perhaps his most striking trait. In matters of *mores,* morals, and politics he drew sharp lines between "good" and "bad" in a manner that set him off against the sophisticated groping and wavering of our day. Opinions became convictions with him, to be proclaimed and defended.

This was equally true in his scientific thinking. He had little patience with the polite inertia with which ideas are too often met today in scientific circles, as if nothing were ever worth attacking or defending. He spared no pains to inform himself on all aspects of a controversial subject. Then he made a decision. Fully convinced of the thoroughness and impartiality of his analysis, he considered the position reached as final. He rarely, if ever, changed it.

Yet Johnson welcomed opposition if it was free from personal motives. Years ago, the writer met him in the field on one of his cross-country trips with a group of graduate students, to defend the published interpretation of a certain region which one of Johnson's students tried to explain in a radically different way. At the time the writer did not know that Johnson had spent many days in this region the year before and that in his graduate seminar during the preceding winter he had leaned heavily toward the new interpretation. For three days the argument was carried on in the field. The writer spoke his mind plainly and emphatically before the man he had known so far almost solely by reputation, who was ten years his senior. I insisted at the start that it was useless to apply physiographic reasoning to a problem that was essentially of a stratigraphic nature, and that it was evident that neither Johnson nor his student possessed the detailed knowledge of the local stratigraphy and fossils needed in this case. This was as uncomfortable a charge as any man of Johnson's standing could face in the presence of his own students. Yet he admitted at once that it was justified, with that honesty which only a truly great

man can achieve. Like a student, Johnson asked to be shown the critical details over and over again with charming simplicity and directness. At intervals, and again at the end of the trip, he summed up the pros and cons, leaving it to everyone to attach his own weights to each. There was no victory on either side, but there was a superb demonstration of fairness and grace in a controversy. What might have led to bitterness ended in a warm friendship.

The steadfastness with which Johnson adhered to his convictions, he showed in his human relations. His judgments of men, favorable or unfavorable, were as nearly immutable as his other convictions. He watched over his graduate students with paternal interest and provided for them the best be could secure. Friendship was to him a sacred obligation to which he lived up with touching consideration.[9]

Johnson was intimate with but very few. This was due partly to his inflexible attitude toward social customs of which he disapproved, but more to an air of formality which surrounded him as an essential part of the deliberate pattern into which he had fashioned his life. If he did not achieve intimacy, his integrity, his great ability, and his loyalty won him the affection and admiration of those who knew him well.

In fact, this man of great power of mind and will needed for his sense of achievement the tangible approval, if not the praise, of his fellow men; for intellectual satisfaction, the give-and-take of kindred minds; and for happiness, the warmth of true affection.

With the *naiveté* of a high school athlete who exhibits his trophies, Johnson framed and displayed on the walls of his office and of adjoining rooms every document that told of honors received. He called this exhibit "my vanity corner." To him they were tangible measures of hard-earned progress on the road to "success." Not every man needs them. He did.

The deeper human need of an active mind that craves companionship in its wide interests and response to his joy in beauty

[9] Johnson loved good company and entertained with grace and gusto. He enjoyed telling and hearing stories. He had a real sense of humor, though he did not readily turn it on himself. He took himself too seriously.

was filled in Johnson's life in a unique way through his marriage with Alice Adkins. She was such an integral part of thirty-five years of Johnson's life that to understand him, one must know her.

When Alice became engaged to Douglas, she still could see dimly. She tried every medical advice. An operation promised restoration of sight in one eye, but ended in total blindness.

With admirable determination and courage, Alice rose above her affliction. She learned to walk with such sure-footed confidence that strangers discovered with surprise that she was blind. She became a gracious hostess who knew how to put newcomers at their ease. "I cannot see you," she would say, "you will have to make advances."

She had a receptive and tenacious memory and loved the beauty of words. She wrote sensitive poetry and was a brilliant conversationalist. She knew French, Italian, and German. She played the piano and used the typewriter.

All who knew her, admired her; above all, they respected her boundless courage. Undaunted, she went with her husband wherever he traveled—whether it be up the rocky slopes of Mt. San Francisco or through the art galleries and cathedrals of Europe. She accompanied him on trips in most of the United States and through parts of three other continents. Wherever they went he took pains to convey to her in vivid words what lay before them, training and refining unwittingly his natural gift of apt and graphic speech and his quick grasp of what is essential in what the eye beholds.

The joint life of Alice and Douglas ripened to a companionship of epic quality. She released in him a tenderness and chivalry that few men can achieve. It was as delicate and sincere as it was immutable, and she quickened and spurred in him all the powers of mind and heart.

On October 11, 1938, Alice died. Douglas stood tragically alone.

In 1942, after a summer spent in part in the field, in New Mexico and Arizona, Johnson suffered a severe heart attack, while visiting his beloved Granville. Recovery was slow, but

living strictly according to doctor's orders, he regained strength steadily. His confidence rose and when his physician gave him at least eight years to live, he decided to marry again. While visiting a niece in Louisville, Kentucky, he had met Edith Sanford Caldwell, the widow of Dr. M. A. Caldwell, who had been head of the department of psychology in the University of Louisville. This acquaintance had become a friendship which held promise of a happy companionship. On September 8, 1943, Edith Caldwell became Mrs. Johnson. Proudly Johnson brought her to New York and introduced her to his circle of friends. Three months later they started south to spend the winter in Florida. On the way down, their train was involved in a wreck which cost the lives of at least eighty-two fellow passengers. "There was only one car between us and disaster," he wrote. Two months later, on February 24, 1944, he succumbed to a heart attack in his winter quarters at Sebring, Florida.

APPENDIX

I. Membership and offices held in learned societies

1. U. S. A.

a. General

National Academy of Sciences, elected 1932
National Research Council, Division of Geology and Geography
Member at Large, 1917-23, 1927-29
Committee on the Development of the Geographical Societies, 1920-23
Committee on Shoreline Investigations, Chairman, 1923-26
Member, 1927-33
Committee on the International Geographical Union, 1928-37
Chairman, 1933-37
Committee on Fellowships, 1931-37
American Philosophical Society
Councilor, 1942-44
American Academy of Arts and Sciences
New York Academy of Sciences
Vice-president, Section of Geology, 1916-17
Councilor, 1919-21, 1927-29, 1940-42
American Association for the Advancement of Science
Vice-president, Section E, 1931

b. Geography

Association of American Geographers
President, 1928
American Geographical Society
Geographical Society of Philadelphia

c. Geology

Geological Society of America
Vice-President, 1927, 1940, 1941
President 1942
Councilor, 1943

2. Foreign

a. General

British Association for the Advancement of Science
Vice-president and guest, 1928

Académie Royale Serbe
(Corresponding member)

b. Geography

Société Royale Belge de Géographie
(Corresponding member)
Bordeaux Geographical Society
(Honorary member)
Société de Géographie de Beograd
(Honorary member)
Svenska Sällskapet för Antropologi och Geografi
(Foreign member)
Geographical Society of Finland
(Foreign member)
Société Russe de Géographie
(Corresponding member)

c. Geology

Geological Society of London
(Foreign member)
Société Belge de Géologie
(Corresponding member)
Geological Society of China
(Coresponding member)

3. International

International Geographical Congress, Paris, 1931
President, Section of Physiography and Geography
International Terrace Commission
President, 1934-38

II. Special Honors

1. Honorary degrees

University of Grenoble, France, 1924
(Docteur, honoris causa)
Columbia University, U. S. A., 1929
(Sc. D.)
University of Nancy, France, 1932
(Docteur, honoris causa)
Denison University, U. S. A., 1932
(Sc. D.)
University of Montpellier, France, 1933
(Docteur, honoris causa)
University of New Mexico, U. S. A., 1942
(L.L.D.)

2. Medals and prizes

Walker Memorial Prize,
Boston Society of Natural History, 1906
Janssen Gold Medal,
Société de Géographie de Paris, 1920
Elisha Kent Kane Gold Medal,
Geographical Society of Philadelphia, 1922
Medal of the University of Nancy, 1924
Gaudy Medal,
Société de Géographie Commerciale, Paris, 1925
A. Cressy Morrison Prize,
New York Academy of Sciences, 1924 and 1930
Cvijic Medal,
Geographical Society of Belgrade, 1935
Cullum Medal,
American Geographical Society, 1935

3. Decorations

Chevalier,
Légion d'honneur, France, 1924
Cross of St. Sava with star,
Yugoslavia, 1934

KEY TO ABBREVIATIONS IN BIBLIOGRAPHY

Am. Geog. Soc. Bull. = American Geographical Society, Bulletin.
Am. Geol. = American Geologist.
Am. Jour. Sci. = American Journal of Science.
Am. Phil. Soc. Proc. = American Philosophical Society, Proceedings.
Am. Scen. Hist. Pres. Soc. = American Scenic and Historic Preservation Society.
Am. Shore Beach Pres. Assn. = American Shore and Beach Preservation Association.
Ann. Géog. = Annales de géographie.
Assn. Am. Geog. Ann. = Association of American Geographers, Annals.
Boston Soc. Nat. Hist. Proc. = Boston Society of Natural History Proceedings.
Bot. Gaz. = Botanical Gazette.
Bull. Geog. Soc. Phila. = Bulletin, Geographical Society of Philadelphia.
Columbia Univ. School of Mines Quart. = Columbia University School of Mines Quarterly.
Cong. Int. Géog., C.R. = Congrès International de Géographie, Comptes Rendus.
Denison Univ. Sci. Lab. Bull. = Denison University Science Laboratory, Bulletin.
Ecol. = Ecology.
Econ. Geol. = Economic Geology.
Geog. Jour. = Geographical Journal.
Geog. Rev. = Geographical Review.
Geog. Soc. Phila. = Geographical Society of Philadelphia.
Geol. Soc. Am. Bull. = Geological Society of America, Bulletin.
Geol. Sur. N. J. Bull. = Geological Survey of New Jersey, Bulletin.
Harvard Grad. Mag. = Harvard Graduate Magazine.
Hist. Out. = Historical Outlook.
Int. Geog. Cong. Rpt. = International Geographical Congress, Report.
Jour. Geog. = Journal of Geography.
Int. Geol. Cong., C.R. = International Geological Congress, Compte Rendu.
Jour. Geol. = Journal of Geology.
Jour. Geom. = Journal of Geomorphology.
Nat. Hist. = Natural History.
Nat. Res. C. Bull. = National Research Council, Bulletin.
N. Y. Acad. Sci. Annals = New York Academy of Sciences, Annals.
Pop. Ed. = Popular Educator.
Pop. Sci. Mo. = Popular Science Monthly.
Rice Inst. Pam. = Rice Institute Pamphlet.
Sci. Mo. = Scientific Monthly.
Tech. Quart. = Technology Quarterly.
U.S.G.S. = United States Geological Survey.

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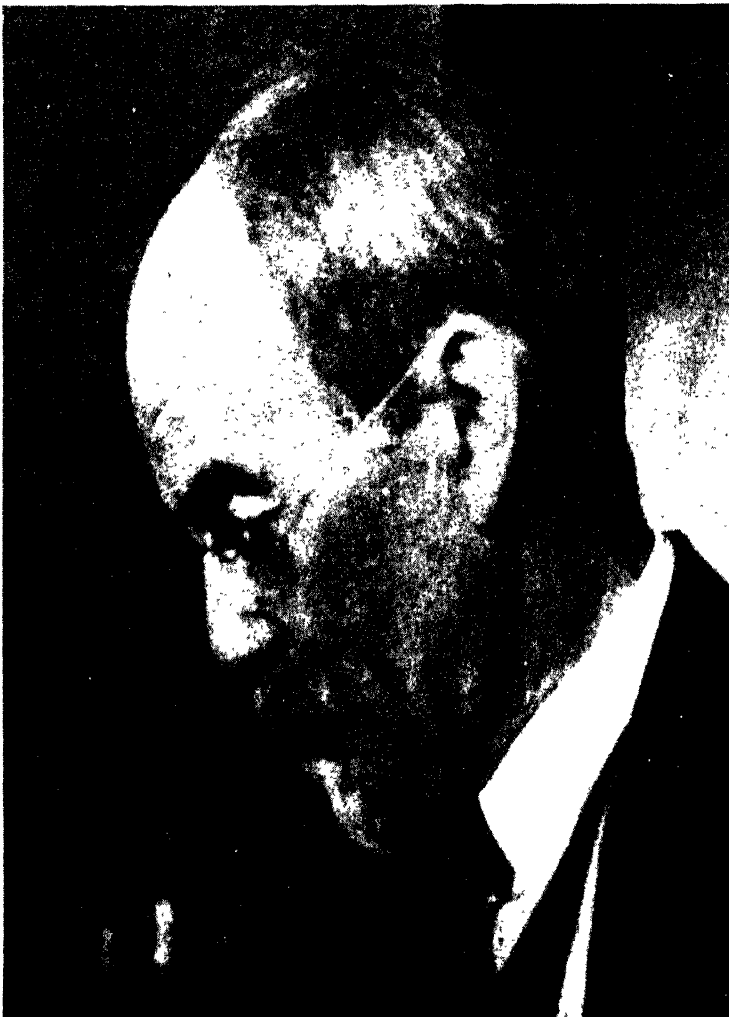

John J Abel

NATIONAL ACADEMY OF SCIENCES

OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XXIV—SIXTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

JOHN JACOB ABEL

1857–1938

BY

WM. DEB. MACNIDER

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1946

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PART I

> "The value of a man is not measured by the truth he possesses, but rather by his sincere effort to discover truth. For it is not through the possession of truth, but more by search for it that his powers are widened, those powers which conduce to evergrowing perfection. Possession makes for tranquility, laziness and conceit. If God, holding in His right hand the complete embodiment of truth and in His left the unswerving and ever alert search for truth, even though this search be fraught with a constant and unremitting erring, should say to me: 'Choose!', I would humbly embrace His left.and say 'Father, give! for real truth is Yours alone'"
>
> . . . Lessing.

Such was the life, the labor and the religion of John Jacob Abel, who was born near Cleveland, Ohio on the 19th of May, 1857 and died in Baltimore at the Johns Hopkins Hospital May 26, 1938. He died as he had lived, acutely conscious of all about him, thoughtful of his medical associates and attendants, with full knowledge of his. basic ailment and with a yearning for his laboratory. Only a few hours before his death he discussed and planned in his usual animated fashion with one of his associates a continuation of their work with tetanus toxin. Likely few men in science at the age of 81 have held fast to those characteristics of mind and of spirit that partake of youthfulness and insure a continuation of creative work at a high level of effectiveness.

But little is known of Professor Abel's forebears. His family came from the Rhine Valley of the Palatinate, stock which has given to this country through its sincerity of purpose leaders in industry as well as in science; people with a desire for success dependent upon and assured through toil as a way of life and

as a means to secure specific training for a desired objective. This origin of the Abel family coupled with his years of training under and intimate association with the leading scientific men in Germany in the general field of the biological sciences made it difficult for Professor Abel to accept America's conflict with Germany in World War I.

There was in the young Abel that something which as a youth without hereditary example directed him into pathways of understanding through studiousness. Following the usual high school experience Abel entered the University of Michigan and received his Ph.B. degree in 1883. This stay at Michigan was not one of ease and continuity of purpose for during his undergraduate years there was an interim of three years when he served as principal of the high school at La Porte, Indiana. Acting in this capacity he dispensed information in mathematics, Latin, chemistry and physics. Even at this early stage of his career his understanding was of such a diversified order that he was able to give instruction in subjects as widely separated in their disciplines as Latin and chemistry. This breadth of interest in the things of the mind remained with Professor Abel throughout his life. He distrusted abstracts but went to original sources for his information. The Greek language held a lasting charm for him.

While teaching at La Porte, fate was kind enough through chance to have another teacher, Miss Mary Hinman, of New York State, become an instructor in these schools. Likely other than the impelling desire within Professor Abel for intellectual advancement towards perfection, Miss Hinman, "the very sweet, mild, little lady with a great deal of force" had the greatest influence on his life and development and in no small measure made this development possible. These young people were married on July 10, 1883. Their fortunes for life became as one. Their combined savings were for his intellectual yearnings and these desires were of an Old World order of excellence, not one master for specialized instruction but many, both at home and especially abroad, fitted his mind for teaching and for a diversity of research interests. This training resulted in the

development of an order of mental equipment which permitted a research thought to be followed by the use of specialized techniques for its investigation. He was blessed by such thoughts and the ability to test their worthwhileness.

At the University of Michigan, Abel had experienced the power and the inquisitiveness of mind of a group of eminent scholars who relished the instruction of undergraduates as well as those students at a more advanced period of training. This is as it should be. Such a student-teacher relationship not infrequently in the silences precipitates out as a determination the best within a student and both stimulates him to go forward in his training and sustains him in difficult periods of such accomplishment. Likely under the operation of this influence and with the insistence and support of Mrs. Abel, having decided on an educational career, he went to the Johns Hopkins University, the first institution in this country primarily established for graduate study, and spent a year under the guidance in physiology of Newell Martin. From this experience of a graduate order of thought and training Dr. and Mrs. Abel went abroad in order that he might have seven years of such study under the leading masters in the medical and allied sciences in Europe. During the long period of intellectual fellowship with training which would warrant at its culmination the Doctorate in Philosophy, he chose the designation Doctor of Medicine, which degree was awarded him by the University of Strassburg in 1888. There is an element of surprise in such a choice for as one knew Dr. Abel he was idealistically constituted both by disposition and training to teach, to direct and to do research and not to teach or to practice medicine as it was done in America in the early eighteen-nineties. He likely realized the necessity for him to possess the M.D. degree in order by practice to make a living in this country and he likely also determined that should this be his fate he would practice medicine with a scientific order of exactness and participate in investigation of a clinical nature as was the custom by the best clinicians in continental Europe. His medical training was of an exceptionally broad and high order of excellence.

The seven years devoted by Dr. Abel to his studies in Europe must have been of an ideal nature. The hurry and strain of accomplishing a certain designated and regimented curriculum leading to a specified degree at the end of a certain time limit did not exist. He had time for thoughtfulness and an opportunity to ripen in wisdom through unhurried training. He picked with rare judgment those scholars of eminence in the different divisions of understanding in which he desired information and with a certain leisure proceeded with his work which at a very early stage expressed itself in various research undertakings. This character of his work gave him not only guidance by but intimacy with these men of exceptional ability, and quite naturally such associations made for mutual respect and lasting friendships. Even in these early days of 1884, when Abel was only 27 years of age, one of his lasting characteristics was in evidence: the thoughtfulness with which he came to a decision and the modest ease with which he proceeded to live his life. "From 1884 to 1886 he was in Leipsig, studying physiology under Ludwig and von Frey, histology under His, pharmacology under Boehm, pathology under Strümpell and inorganic and organic chemistry under Wislicenus. At this time he completed the basic work on his doctor's dissertation in Ludwig's laboratory. The winter semester of 1886-87 was spent in Strassburg under Kussmaul in internal medicine, and under V. Recklinghausen in pathology and infectious diseases. The following summer semester he studied at Heidelberg with Erb in medicine and Czerney in surgery. During the summer vacation he attended clinics at Würzburg to return to Strassburg in 1887-88 for study under Kussmaul, Naunyn, Hoppe-Seyler and Schmeideberg. It was Schmeideberg who first aroused Abel's interest in pharmacological research, particularly in its chemical aspects." The stimulation of this basic interest by Schmeideberg had much significance for it was to be in this domain of chemistry, tissue chemistry as pharmacology, that a large part of Abel's research life was to be spent. After Abel received his Doctorate in Medicine from Strassburg and likely with the conviction, perhaps against his will, that on his return home he

must practice medicine for a livelihood, he went to Vienna for a year to work with Nothnagel and others in clinical medicine. This was a transitory departure from his laboratory studies, for the period 1889-90 found him a student in the biochemical laboratory of V. Nencki in Berne. Here his time was completely given to biochemical research with the completion of an investigation on the "molecular weight of cholic acid, cholesterol, and hydrobilirubin." It was at Berne that Abel made the acquaintance of and established a lasting friendship with Cushny, who was then a student in Kronecker's laboratory and who was later to follow Abel as Professor of Pharmacology at Michigan. This period at Berne under V. Nencki would appear to have fixed Abel's scientific interest in the domain of biochemistry and pharmacology as much as he may have feared the necessity for his participation in the practice of medicine. With such a desire for basic scientific research it was not only fortunate for him but a blessing to science that in the summer of 1890, upon the recommendation of Schmeideberg to Victor C. Vaughan, the latter offered Abel the chair of Materia Medica and Therapeutics at the University of Michigan. This he accepted but before going to his first academic post he spent a period in Ludwig's laboratory working with the eminent biochemist, Drechsel. One may be permitted to visualize the joy and satisfaction which had come to Professor Abel and Mrs. Abel alike to have the opportunity after such a period of carefully planned training to be called to the headship of a scientific department in one of America's great universities and here to be able to create in a modern sense the first Department of Pharmacology in America. Soon after Professor Abel's entering upon his duties at Michigan he wrote a letter to C. W. Edmunds in which he stated that "here at Ann Arbor I was given the opportunity of starting the first professorship in pharmacology in the United States, whose holder should devote himself entirely to giving students the best possible instruction by means of lectures, demonstrations and quizzes in the manner in which my European teachers (Schmeideberg and Boehm) had long carried on their work. All my energy which was not given to this kind of instruction

to students was devoted to research work and to arousing the enthusiasm in others for it. . . . There was no laboratory of any kind at my disposal. There was not a scrap of apparatus, not even a test tube, a flask or a beaker." Such a lack of physical necessities for work was overcome until they were secured by his optimism and enthusiasm which remained with him until the day of his death. He had that which he most cherished, an opportunity to work, and with this spirit he went forward to equip in a meagre fashion a workshop which might be designated laboratory and to proceed with his teaching and research. This course which he pursued, not as a difficulty but as a challenge, should be known and taken to heart by many of the younger experimentalists of the present who would appear to consider apparatus as such to be responsible for research and thought, enterprise and reason on the part of the investigator to be only of secondary importance.

Professor Abel's tenure of the chair of pharmacology at Michigan was of short duration. In the early nineties President Gilman, of the Johns Hopkins University, was busy with his desire to establish at this institution a different order of medical school from that then in operation in this country, a school which would have for one of its characteristics a scientific, research point of view, not only in teaching medical students but as applied in the treatment of disease. The so-called laboratory or preclinical subjects of the medical curriculum were to assume a university order of excellence in their development and operation and the clinical departments were to find themselves through the laboratory and at the bedside and away from didactic teaching. An order of medical instruction first developed in continental Europe was to appear in Baltimore. Such scientific idealism for medicine must have had a strong appeal for Professor Abel, who had experienced in his training this type of inquisitiveness as medical instruction. The result was that, responding to a letter from Osler, Abel accepted the Professorship of Pharmacology at this new order of medical school and assumed his duties in this capacity for the year 1893. At this time is was likely fortunate for biological chemistry that this

subject fell under Professor Abel's care for he was not in favor of such a combination of intellectual interests but had at heart the free development of biological chemistry and pharmacology as entities. This was shown in 1908 by the separation of these two subjects with the creation of two independent departments, with Walter Jones serving as the first professor and head of the department of biological chemistry at the Johns Hopkins Medical School.

Professor Abel's tenure of posts at the Johns Hopkins Medical School lasted over a period of forty-five years. From 1893 to 1908 he was responsible for both pharmacology and biological chemistry. From 1908 to 1932 the chair of pharmacology held his undivided attention. At the age of 75 he retired from this professorship, which was filled by his eminent pupil, Professor E. K. Marshall, Jr., in order that Professor Abel as Director of Endocrinological Research might spend his entire time in investigations in this area of understanding. He held this position with great interest and activity at the time of his death.

In this brief account of the life of Professor Abel no attempt will be made to discuss in any detail of an analytical order his research interests and accomplishments. This has in large measure been done by one of his former pupils and associates, Dr. Carl Voegtlin of the National Institute of Health. From his statement I have drawn freely. As one studies these contributions, commencing with his dissertation at Strassburg in 1888 to his final paper dealing with his new interest in the method of the transmission of tetanus toxin to the central nervous system which appeared in 1938, one is impressed by his abiding interest in the chemical manipulations of animal tissues, his impelling desire to understand such mechanisms, not only as processes of life but his desire to obtain in pure form those chemical bodies responsible for tissue activity, especially the activity of the endocrine glands. From his early paper on the isolation of ethyl sulfide from the normal urine of dogs, or as was the case in his work with Drechsel in Ludwig's laboratory, on the occurrence of carbamic acid in alkaline horse urine through the years of work on the suprarenal medulla, the isolation of

amino acids from blood by vividiffusion and the period of work on the pituitary, the same basic order of thought shows itself; to understand the chemical constitution of tissues and the interplay of such bodies in the living organism as an expression of a normal, balanced and related life. With few exceptions he did not become experimentally concerned with the chemical constitution of substances foreign to the animal organism. Notable exceptions to this statement are his work with Rowntree on the trypanocidal action of certain antimony-arsenic compounds, the fate and elimination of the phthaleins in animal tissues and his final work with Firor and Chalian on tetanus. Perhaps he saw an ultimate day for pharmacology when tissue imbalances of function as the symptoms of disease, as departures from the balanced normal life and tissue disintegrations as chemical alterations would be modified back towards the established normal by a replacement of those chemical bodies, normal for various tissues which had been altered or depleted by the strain of tissues to cope with a too severely changed environment.

Even a cursory review of Professor Abel's papers would leave no doubt in the mind of anyone that his dominant interest was the isolation in pure form of the hormones of several of the glands of internal secretion. As early as 1895 this interest manifested itself in his studies on the active element in the thyroid gland. In 1894 Oliver and Schafer had discussed the blood pressure raising power of adrenal extracts. It was at about this date that Abel turned his attention for over a period of ten years to an attempt to isolate the hormone of the adrenal medulla and it was this work even though not carried to complete fulfilment that gave him an international reputation of the first order. A somewhat detailed account of this work is appropriate in the present biographical record on account of various interpretations made concerning it which were very fortunately set at rest by a statement made at a later date by Professor Abel. His first paper on this subject was published with Crawford in 1897, in which he described the isolation of a benzoyl derivative of the active principle. The actual isolation of the active principle as such and the determination of its chemical

structure are questions of much biochemical and historical significance for they are concerned with the first isolation of such a body from an endocrine gland, and are here recorded in Professor Abel's own words as stated in his Willard Gibbs Lecture of 1927. "On decomposing this benzoyl derivative with hot dilute sulfuric acid in an autoclave we obtained the active principle in the form of a sulfate which possessed the characteristic physiological activities of suprarenal extracts and reacted, furthermore, with a series of chemical reagents in a manner that is quite specific for such extracts and limited to them. The principle as obtained by saponification of the benzoyl derivative was thrown out of its solution by means of ammonia in the amorphous state and was shown to be a weak base. A picrate, a bisulfate and other salts of it were prepared, all of which were shown to possess a high degree of physiological activity. An acetyl derivative, a phenylcarbamic ester and other derivatives were also prepared and certain degradation products of the base were isolated and studied. . . . The elementary composition of the base was established by analysis of several derivatives, including the sulfate, and was stated to be represented by the formula $C_{17}H_{15}NO_4$. After I had completed the above described investigation and while I was still endeavoring to improve my processes, I was visited one day by the Japanese chemist, J. Takamine, who examined with great interest the various compounds and salts of epinephrine that were placed before him. He inquired particularly whether I did not think it possible that my salts of epinephrine could be prepared by a simpler process than mine, more especially without the trouble and in this case wasteful process of benzoylating extracts of an animal tissue. He remarked in this connection that he loved to plant a seed and see it grow in the technical field. I told Takamine that I was quite of his opinion and that the process could no doubt be improved and simplified. At this very time, also, V. Fürth had just prepared an amorphous, highly active, indigo-colored compound of the active principle which he named suprarenin, but no analytical data were given and no empirical formula for his principle was established. Takamine prepared supra-

renal extracts more concentrated than mine and without first attempting to separate the hormone from its numerous concomitants by benzoylating or otherwise, simply added ammonia —the reagent that I had so long employed—to his concentrated extracts, whereupon he immediately obtained the native base in the form of burr-like clusters of minute prisms in place of my amorphous base. I have often been asked why I had not myself attempted to solve the problem in this very simple fashion. The truth is that I had tried to do so but always found that the dilute extract tested simply turned pink in a short time on the addition of ammonia without depositing the base either crystalline or amorphous. Inasmuch as very dilute solutions of the salts obtained by me on saponifying the benzol derivatives always gave a precipitate with ammonia, I fell back on the hypothesis that other constituents of the impure extracts prevented its precipitation by ammonia from my dilute native extracts—an erroneous assumption. Takamine's success was due to the employment of ammonia on very highly concentrated, though impure extracts. . . . The efforts of years on my part in this once mysterious field of suprarenal, medullary biochemistry, marred by blunders as they were, eventuated, then, in the isolation of the hormone not in the form of the free base but in that of its monobenzoyl derivative." This extremely important and frank statement of the chemical birth of a new era in the understanding of tissue activity portrays as no words could the industry persisted in to the point of physical exhaustion, the frankness, the complete honesty untouched by jealousy or recrimination of a nobleman in the domain of science. Such a statement would however be only a part truth if mention were not made of the great disappointment at times almost of an incapacitating character experienced by Professor Abel in not having actually carried the epinephrine work to its final point by obtaining the crystals and determining their structural formula. The indomitable spirit of the man overcame this disappointment to go forward for years to a variety of important discoveries which culminated in his isolation with the aid of Geiling of crystalline insulin.

Between the epinephrine and insulin periods of Professor Abel's investigations a wide variety of laboratory adventures were undertaken. Such a diversity of interests and accomplishments were made possible by the broad, and at the same time thorough, training to which he had subjected himself in the days of his studies in Europe. In 1907 Abel and Ford published their paper on the poisonous principles in Amanita phalloides and in 1909 the first of a series of communications appeared by Abel and Rowntree of their studies on the phthaleins from which work sprang the now universally used test for renal function as developed by Rowntree and Geraghty and the more recently developed test for liver function by Rosenthal. It was also at this period of his investigations that Abel and Rowntree became interested in chemotherapy with the production and experimental use of an antimony and also antimony-arsenic compounds in the treatment of trypanosome infections, granuloma inguinale and Bilharzia infections. Somewhat later than this Abel and Barbour and Abel and Turner commenced their studies on the ability of acid fuchsin to produce tetanus in the frog and of the influence of various procedures such as fatigue and of ablation of different areas of the frog brain on the rapidity of development and the severity of such seizures. This work led to their proof of the influence of the lymph hearts in a cardiectomized frog in distributing such agents to the central nervous system. In 1912 there appeared extensive studies by Abel and Macht on the isolation of epinephrine in pure form from the secretion of the parotid gland of the tropical toad, Bufo agua and shortly thereafter a second crystalline body, Bufagin, with a digitalis-like action was obtained from the same secretion. Such researches as important and as significant as they were came rather as an interlude in Abel's basic interests in the chemistry as such or as pharmacology of bodies normal for animal tissues. This interest and his inventiveness were now expressed by his development of an apparatus consisting of a series of celloidin tubes supported in a glass container and surrounded by a dializing fluid which could be introduced into the circulation of an animal so that hirudinized arterial blood would enter at one

connection and return after dialysis through an appropriate venous connection. Such a vividiffusion apparatus, the so-called artificial kidney, was developed by Abel, Rowntree and Turner and first demonstrated at the Physiological Congress in Groningen in 1914. By the use of this mechanism several bodies such as ethyl-sulfide, urea, lactic and B- oxybutyric acids were obtained from dog blood. Of the greatest importance was the first isolation by the use of this apparatus of an amino acid from blood. This order of investigation quite naturally led to the studies of Abel, Rowntree and Turner on plasmaphaeresis, very early work on the regeneration of plasma proteins and in association with Pincoffs and Rouiller and later with Geiling to the study of various bodies of protein structure and also histamine-like bodies in blood. From such studies the question presented itself as to whether or not the active principle of the posterior lobe of the pituitary was a specific entity in the form of a hormone or whether it was an albumose-like substance common for many tissues. The investigations of the posterior pituitary which were to follow brought Abel back to his great fundamental interest in the chemistry of tissue products. These investigations which centered around the posterior pituitary lasted for a number of years and were to be followed by his clean-cut major research contribution accomplished with Geiling in 1927 on the isolation of crystalline insulin and the proof that the insulin effect was dependent on the action of the crystalline body and not to a hormone adsorbed to the crystals.

In 1932, at the age of 75, Professor Abel retired from the chair of pharmacology at the Johns Hopkins Medical School and became Director of the Laboratory for Endocrine Research. At this age, with a brilliant mind, a charm of spirit but with a weakened body, he proceeded with youthful enthusiasm to commence his research in an entirely new domain of investigation, the transmission to and localization of tetanus toxin in the central nervous system.

From this sketch of the major research activities of Professor Abel's laboratory one may inquire as to his fondness for and ability to teach undergraduates and to direct that constant flow

of inquiring minds of a more advanced order of learning that came to his laboratory. He was a great teacher but not in the usual sense accepted for the successful teacher in this country. His laboratory was not pedagogically regimented and likely not until the days of Lamson was it adequately equipped in a mechanical fashion for satisfactory medical student instruction, but over and above such a requirement this laboratory experienced through Professor Abel his abiding interest in students, his enthusiasm for understanding and his suggestiveness as to how chemical bodies might influence tissues. There were no didactic, dogmatic statements but in such young people he registered the spirit of inquiry and a desire for proof which lasted with them and carried such minds to original sources for information and to animals for experimental verification. These men and women left his laboratory with thoughtful inquiring minds and not with crania packed by detached unverified statements.

Professor Abel was opposed to specialized academic degrees in the medical sciences as the doctorate in pharmacology. He did not hold a seminar in this subject. He did much more in an intimate, personal fashion. The informal gatherings for lunch with his laboratory staff, advanced students and those guests working in his laboratory with or without his guidance was the hour, the high-noon for suggestion, criticism, reply and encouragement when ideas were freely exchanged and plans developed for the investigation of various problems. At such informal gatherings eminent scientists from at home or abroad were often present and such breakers of bread at this table of thoughtfulness found themselves on the fine and common level of brothers in science. Thus Professor Abel's instruction to graduates and also undergraduates was not formalized. He imparted a yearning for understanding by his precept, by his faith in toil, by suggestion as to procedure which came from his broad understanding of biological phenomena, especially of a chemical order. Such a guide into the unknown with a sincere interest in and a kindly consideration for others brought an unusually large number of students to his laboratory either

for training in order to become pharmacologists or as part-time assistants. Ranking next to Professor Abel's research contributions is this influence which he exerted in the development of a number of departments of pharmacology in this country and abroad and through his students in divisions of understanding other than pharmacology. The names of Crawford, Aldrich, Hunt, Voegtlin, Loevenhardt, Halsey, Brown, Amberg, Rowntree, Keith, Chen, Pincoffs, Rouiller, Turner, Barbour, Lamson, Macht, duVigneaud, Evans, Marshall, Beiter and Geiling are only a few of the individuals who came under the influence of Abel's laboratory.

Professor Abel was first and foremost a student gaining information through experimentation. His time was spent in the laboratory and was not to any extent complicated by attendance at a variety of scientific gatherings and yet one of his main contributions to science was through his ability to organize such societies and establish journals connected with them for the outlet of their specialized deliberations. In the fall of 1895 he suggested to President Gilman, of the Johns Hopkins University, the need for an American Journal to take care of the ever-increasing amount of material developing in this country in the general domain of experimental medicine. At Gilman's request a plan for such a journal was presented by Abel to the medical faculty which resulted in the establishment of the Journal of Experimental Medicine, with Dr. William H. Welch as the first editor-in-chief. This journal was a success from its first issue in 1896. Encouraged by this outcome and realizing the increase in this country of investigations of a biochemical order, Abel elicited the interest and aid of his friend, Dr. C. A. Herter, Professor of Pharmacology at Columbia University, for the establishment of the Journal of Biological Chemistry. In this instance Herter not only furnished his interest but his financial support for the venture so that with Abel and Herter as joint editors and Professor A. N. Richards as associate editor, the first issue of this publication appeared in 1905. In 1906 Professor Abel called a meeting of a group of biological chemists in New York City which resulted in the formation of the

American Society of Biological Chemists. The first meeting of this society was held in 1907 at which time Abel presented one of the several contributions. The same order of reasoning that specialized thought should come before like-minded individuals and have a specialized outlet in the form of a journal led Abel in 1908 to the establishment of the Society for Pharmacology and Experimental Therapeutics, and a year later to start through the publishing house of the Williams and Wilkins Company the Journal of Pharmacology and Experimental Therapeutics. At first this was a private venture being incorporated in the name of Abel, Hunt and Voegtlin. At a much later date, 1934, this journal became the official organ and financial responsibility of the Society For Pharmacology and Experimental Therapeutics. It was the pleasure of the American Society to request the British Pharmacological Society to become associated with it and in this tangible fashion to strengthen the scientific bond between the two countries. Acting in the capacity of the organizer of scientific societies as well as the instigator for various journals of this nature it would be difficult to evaluate the ever-expanding influence of this simple and learned man of the laboratory.

In writing such a designatedly brief biographical sketch of Professor Abel it has not been difficult to enumerate many of his attainments for they are of such an order of eminent worthwhileness that they stand alone, isolated, for anyone to chronicle and evaluate. As notable and as definite as are many of these contributions, the attribute which made them possible, which made other men of a similar nature of scientific integrity hold fast to the desire to so live and work as to be worthy of the Abel tradition, was that intangible, perhaps nonscientific (who knows?) Something that we may designate, as did J. S. Haldane, spirit. If it be spirit it defies definition, but it was good and honest and thoughtful and rigorous in its exaction for truth through established evidence. His ideal as a man of science was Carl Ludwig, under whom he worked and to whom he felt greatly indebted. In speaking of his great teacher as a guide for the investigation of life, Professor Abel used words first

spoken by Socrates, and these words may be in turn applied to him. "A man whose desires are drawn towards knowledge in every form and who is therefore absorbed in the pleasures of the soul—one who is harmoniously constituted and who is not covetous or mean or a boaster or a coward and can never therefore be unjust or hard in his dealings—he has no secret corner of meanness and is a searcher after and a lover of the truth in all things."

Professor Abel was blessed by the devotion, care and stimulating guidance of his wife, Mary Hinman Abel, over a period of fifty-five years. Mrs. Abel died on January 20, 1938. On May 26, 1938, Professor Abel succumbed to a coronary thrombosis while a patient in the Johns Hopkins Hospital. Three children came from this union, an infant daughter who died in Strassburg, October 30, 1888, and two sons who survive, George H. Abel of Philadelphia and Robert Abel of Boston.

In the preparation of this biographical memoir I must first acknowledge my indebtedness to Dr. Carl Voegtlin for the privilege of the free use of his appreciation of Professor Abel which appeared soon after his death in the Journal for Pharmacology and Experimental Therapeutics, and to Miss Dorothy Wilson, one time secretary to Professor Abel, for the list incorporating the degrees, medals and awards, and the outline of his career. I am also indebted to Professor E. M. K. Geiling for certain letters concerning Professor Abel, and for his as well as the statements concerning Professor Abel by E. K. Marshall, Jr., Paul D. Lamson and W. Mansfield Clark. To Drs. Samuel Amberg and L. G. Rowntree I am grateful for valuable information contained in personal letters.

PART II

Degrees:

Ph.B., Michigan, 1883
Hon. A.M., Michigan, 1903
Hon. Sc.D., Michigan, 1912
Hon. Sc.D., Pittsburgh, 1915

Hon. Sc.D., Harvard, 1925
Hon. Sc.D., Yale, 1927
M.D., Strassburg, 1888
Hon. M.D., University of Lwów, Poland, 1927
Hon. LL.D., Cambridge, 1920
Hon. LL.D., Aberdeen, 1932

Medals and Awards:

Awarded first Research Corporation Prize, 1925
First award of Lectureship of Kober Foundation, 1925
Award of the Willard Gibbs Gold Medal by the Chicago Section of the American Chemical Society, 1927
Awarded Gold Medal, Society of Apothecaries, London, 1928
Awarded the Conné Medal, New York Chemists' Club, 1932
Awarded Kober Medal, 1934.

Outline of Career:

Principal, High School, La Porte, Indiana, 1879-1880
Superintendent of Public Schools, La Porte, Indiana, 1880-1882
Graduate Student, Hopkins, 1883-1884
Student of chemistry and medicine, 1884 to January, 1891, at:

Leipsig
Strassburg
Heidelberg
Vienna
Berne
Würzburg
Berlin

Lecturer, Materia Medica and Therapeutics, University of Michigan, January to June, 1891
Professor of Materia Medica and Therapeutics, Michigan, 1892-1893
Professor of Pharmacology, Hopkins, 1893-1932 (July)
Professor Emeritus of Pharmacology and Director of the Laboratory for Endocrine Research, 1932 to 1938 (May)
Editor of the Journal of Pharmacology and Experimental Therapeutics, 1909 to 1932 (July)

Memberships in Societies in the United States:

National Academy of Sciences
American Philosophical Society
Fellow, American Association for the Advancement of Science
American Physiological Society
American Chemical Society
American Society of Biological Chemists
Society of Experimental Biology and Medicine
Society for Pharmacology and Experimental Therapeutics

Honorary Memberships in the United States:

Honorary Fellow, New York Academy of Medicine
Honorary member, Association of American Physicians
Honorary member, The Chemists' Club
Honorary Fellow, The Institute of Medicine of Chicago
Honorary member, Philadelphia College of Pharmacy
Honorary member, American Institute of Chemists

Memberships in Foreign Societies:

Foreign member, Dutch Society of Scientists
Foreign member, Royal Society of London
Associate member, Société Royale des Sciences Médicales et Naturelles de Bruselles
Corresponding member, Société de Biologie de Paris
Corresponding member, K. K. Gesellschaft der Aerzte, Vienna
Honorary Fellow, Royal Society of Edinburgh
Honorary member, Kais. Deutsch. Akad. d. Naturforscher zu Halle
Honorary member, Physiological Society of Great Britain
Honorary member, The British Pharmacological Society
Honorary Correspondent, Société de Therapeutique de Paris
Honorary member, Society for Biology of Buenos Aires
Honorary member, Chinese Physiological Society
Honorary Fellow, Société de Chimie biologique
Honorary member, Wiener Biologischen Gesellschaft

PART III

BIBLIOGRAPHY OF JOHN JACOB ABEL

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

Am. Chem. J. = American Chemical Journal
Am. J. Phar. = American Journal of Pharmacy
Am. J. Physiol. = American Journal of Physiology
Am. Med. Assn. Bull. = American Medical Association Bulletin
Arch. f. exper. Path. u. Pharmakol. = Archiv für experimentelle pathologie und pharmakologie
Arch. f. Physiol. = Archiv für physiologie
Ber. d. deutsch. chem. Gesell. = Deutsche chemische gesellschaft Berichte
Ber. d. Kais. Akad. d. Wissensch. in Wein, Mathem.—naturw. Classe, Abth. = Berichte d. Kaiserlichen Akademie der wissenschaften, Vienna, mathematisch-naturwissenschaften, klasse—Abtheilung
Bull. Johns Hopkins Hosp. = Bulletin, Johns Hopkins Hospital
Ind. & Eng. Chem. = Industrial & Engineering Chemistry
J. Am. Med. Assn. = Journal, American Medical Association
J. Biol. Chem. = Journal of Biological Chemistry
J. Chem. Ed. = Journal of Chemical Education
J. Exper. Med. = Journal of Experimental Medicine
J. Indus. & Eng. Chem. = Journal of Industrial & Engineering Chemistry
J. Pharmacol. & Exper. Therap. = Journal of Pharmacology and Experimental Therapeutics
Phila. Med. J. = Philadelphia Medical Journal
Proc. Inst. Med. Chicago = Proceedings, Institute of Medicine of Chicago
Proc. Nat. Acad. Sci. = Proceedings, National Academy of Sciences
Trans. Assn. Am. Phys. = Transactions, Association of American Physicians
Univ. Mich. Rec. = University of Michigan Record
Virchow's Archiv. = Virchow's archiv für pathologische anatomie und physiologie und klinische medizin
Ztschr. f. Physiol. Chem. = Zeitschrift für physiologische chemie

1888

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1890

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1891

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1892

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1894

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1899

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1900

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1901

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1902

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1903

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1905

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1906

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1913

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1917

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1919

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1924

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1925

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1926

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1927

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1928

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1929

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1930

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1934

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1935

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(With Bettylee Hampil.) Researches on Tetanus. IV. Some Historical Notes on Tetanus and Commentaries Thereon. Bull. Johns Hopkins Hosp., lvii, 343-372.

1936

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E. O. Ulrich 1927

NATIONAL ACADEMY OF SCIENCES

OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XXIV—SEVENTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

EDWARD OSCAR ULRICH
1857—1944

BY

RUDOLF RUEDEMANN

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1946

EDWARD OSCAR ULRICH
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I met Doctor Ulrich for the first time when in 1903 he came to Dr. Clarke with a request from the U. S. Geological Survey that geologists of the New York Survey should take him over our Cambrian, Ordovician and Silurian formations. I was assigned to take him over the Cambrian and Ordovician.

So we started out up the Hudson River, stopped at Whitehall, went through the Champlain basin, skirted around the Adirondacks in the north and returned by way of the Black River Valley to Utica where another geologist was to meet us and take Ulrich over the Silurian. The geologist in question had not appeared, apparently afraid of Ulrich's superior knowledge, and I took him also over our Silurian.

Wherever we stopped and I showed him the geologic section, Ed would say: "It is not so!" A hot discussion would always follow. After a few days of steady arguing, I became exhausted and quit disputing. Then Ulrich said: "If you quit disputing, I will go home. I always develop my ideas going out with others and disputing theirs." I then changed my tactics and when we came to a new outcrop, I pretended not to be sure and asked him about it. Ulrich was a born teacher and loved to be asked. He would at once take up the problem and give me full information and I learned a lot that way.

His knowledge of fossils as well as of Paleozoic formations in America was absolutely uncanny and sufficient to crush any adversary. He identified the fossils at sight, I quietly noted down the names and later reidentified the material and found all identifications correct! Likewise, his memory was incredible. I took him years later to sections we had studied on that first trip and he remembered every foot of them.

His unfailing memory and his universal knowledge of the Paleozoic fossils and formations were his most outstanding accomplishments. Add to this his strong health, his ever-fiery enthusiasm and an outstanding scientist without any peer in his field was the result.

Ulrich was largely self-educated. Regarding the fascinating facts of his youth and education, I may refer the reader to the appreciative account written by his life-long friend and colleague, Dr. Ray S. Bassler for the Geological Society of America (Memorial to Edward Oscar Ulrich, Proc. Geol. Soc. Am. for 1944, pp. 331-351.).

I became well acquainted with the problems which were nearest to his heart on a trip my wife and I made with Doctor and Mrs. Ulrich to Europe in 1922, where they attended the International Geological Congress in Belgium. We travelled together to Sweden, where I discovered that Ulrich went there principally for the purpose of looking for his Ozarkian system while I wanted to see the Swedish graptolite shales. Swedish geologists took us around through the interesting and pretty country but to my great sorrow no trace of the Ozarkian could be found. It was a tragedy, made still more harrowing by Mrs. Ulrich's illness.

In Sweden Mrs. Ulrich showed first signs of this illness and in Switzerland she became seriously ill. A Swiss specialist diagnosed the case as much advanced diabetes and told us that she might go any time into a coma. Upon hearing this Mrs. Ulrich asked to return at once to the States, as she did not want to die in a foreign country. So we left, missing our Bohemian trip which I had worked out. Ulrich had to engage a nurse for the trip. He was very fortunate to secure the services of Miss Lydia Sennhauser of Adorf, a graduate nurse of the Red Cross school and a woman of exceptional ability and pleasant character. She stayed with Mrs. Ulrich as long as her entry permit allowed.

Mrs. Ulrich bore her fatal disease with sublime courage and patience until her death ten years later. She had been a schoolteacher in Cincinnati and had been a wonderful helpmate in the years when Ulrich with a small income was struggling to find recognition. She had a pleasant voice and liked to sing when we were among ourselves in the evening. My wife and I have very fond memories of her.

Eddie became a paleontologist when he was seven years old. He found a rock-pile near his home in Covington across the river from Cincinnati, where he was born February 1, 1857, the son of a former French officer and later contractor and builder. The rocks and fossils had been thrown out by the Rev. Henry Herzer, a minister who had made himself a reputation as a collector and student of the local fossils. The minister told him they were "fossils, ancient animals turned to stone." The kind minister instructed the wide-awake boy how to collect fossils and where to find them in the neighborhood. Thus started an interest and a career that never flagged in a long life and enriched American geology and paleontology beyond belief.

Ulrich was original in everything he undertook. He showed that early in life, when he assumed the name Oscar after the hero in one of the early stories he read, and did not go through the various collecting stages of boys, as collecting coins, postage stamps, etc., but remained steadfast to his first interest, Cincinnati fossils.

He attended the public schools intermittently owing to his delicate physique until 1872, when he was able to finish grade-school at the age of fifteen. As, owing to his splendid memory, it was easy for him to keep up his school work, he found much spare time to increase his collection of fossils. After leaving school the excavations for the Eden Park Reservoir of Cincinnati were started. Here was an opportunity for more collecting in the type outcrops of fossils from Eden shale. He applied for a job with the water-works and being tall for his age, secured one as a rodman. He held this job for two years, using his spare time in roaming the Cincinnati hills for fossils. When I went to Cincinnati on a visit, I took my wife over these hills in memory of Ulrich and she enjoyed the beautiful views of the Ohio Valley.

The Reverend Herzer, a trustee of the German Wallace and Baldwin College at Berea, Ohio, was able to get Ed's father to persuade the boy to try for college work. At the age of 17,

in spite of the lack of high school training, he passed the examinations for entrance to the sophomore class of that college. The boy, however, was not at all enthusiastic as he insisted that he was taught too much he did not need and too little that he did. So he turned to other occupations; he became captain of the baseball team and later the science teacher, appreciating his love for geology, put him in charge of the school collection. Training at the college was designed for entrance into the ministry, but this did not appeal at all and he returned home without graduating. Later the college recognized his attainments by conferring upon him the degrees of A.M. in 1886 and Ph.D. in 1892.

The following year his father and his uncle, a physician, prevailed upon him to attend medical school at the old Pulte and Ohio Medical Colleges at Cincinnati. Two winter terms of the medical school were sufficient to bring Edward back to his geology in 1877.

A turning point in Edward's life came in the autumn of 1877, when Dr. R. M. Byrnes invited him to join the Cincinnati Society of Natural History. The following spring the Society, pleased by Edward's enthusiasm, appointed him honorary curator of paleontology. When later the society was enabled by a bequest to buy a commodious building it hired Edward as caretaker in charge at $30 a month. In the following year, when Ulrich published his first scientific paper, he was put in charge of the natural history collections with a small salary, which required the utmost frugality such as fitting up a living room in his father's carpenter shop.

The year 1878 was also important due to the fact that then the 20 year old Charles Schuchert, working in his father's furniture business, commenced to attend the meetings of the Natural History Society and began to bring fossils for identification, thereby beginning his life-long acquaintanceship and later partnership with Ulrich in some important publications. The two inquisitive spirits of Ulrich and Schuchert delved also into other subjects as spiritualism, which then swept the country. As both had good voices, they had rôles for several seasons in local

amateur productions of the Gilbert and Sullivan operas. Ulrich even told me once that he wondered at that time whether he should not develop himself into an opera singer. Fortunately for geology and paleontology he did not follow up that inclination. Ulrich formed at that time the center of a group of young men whose interest in geology and paleontology he developed and who became prominent scientists. Among them should be mentioned especially Ray S. Bassler and John M. Nickles, besides Schuchert.

An interesting episode of Ulrich's life that followed was his appointment as superintendent of the Little Caribou Silver Mines, a camp about twenty miles west of Boulder, Colorado, where some fifty silver-lead prospects were awaiting development. Ed used to tell me about his experiences there, and his boyhood friend Henry Dickhaut who went with him told me more. The story was how once while horseback riding out in the country he was overcome by a blizzard and became so faint that he left the bridle to his horse, which took him to a farmhouse and saved his life. Dickhaut told me that Ulrich discharged a miner, who was a two-gun man and did not work but was kept on the pay roll because everybody was afraid of him. The gunman stayed around in the saloons advertising the fact that he was going to shoot Ulrich at sight. A day or two after Ulrich and Dickhaut met the gunman coming toward them with a couple of cronies. It was a question who could first pull the gun. Dickhaut said in a loud tone to Ulrich: "I bet you two dollars you cannot hit the top of that small spruce tree over there." Ulrich pulled his gun and shot it off. The two parties passed each other quietly and the gunman left the camp. Another time Ulrich saw a miner working awkwardly at the edge of a pit. He took the bar away from the miner to show him how to handle it, slipped and disappeared in the pit. Everybody thought he was killed, but soon they heard him yell to be pulled out. He told me that he saved himself by throwing himself from one side of the pit to the other and fortunately landed on a pile of soft earth. Dickhaut also told me how Ed, when he worked for his father as a carpenter, once crawled out

on a fresh brick wall. The wall began to sway so that everybody yelled at him to lie down quietly, and the fire department had to be called to bring him down with a ladder. His father's language is not preserved for record. These and other adventures characterize Ulrich's audacity which was liable to bring his companions into dangerous positions and brought about some of my own strenuous experiences with him.

The job in Colorado lasted only two years because the ore proved scarce and finally the paymaster ran away to South America. Ulrich came home to his father's shop with new experiences in both geology and life. He continued to live solely through his carpentry work, but found time nevertheless for the description of various local fossils. Schuchert, who had learned to do lithographic work, urged this method of illustration on Ulrich. Both commenced drawing and in time became skilled in producing through the camera lucida natural size and enlarged views of fossils upon the stone and etching the slab afterward for printing.

With these means of illustration Ulrich now published six preliminary papers in the Journal of the Cincinnati Society of Natural History and his three more pretentious papers on American Paleozoic Bryozoa (1882-84). In these articles he described many new genera and species based upon the microscopic structure as shown in thin sections. Although these papers formed the foundation for the modern knowledge of stony Bryozoa they gave their author the reputation of a species maker, an unfair criticism which clung to him and affected his life for the next twenty years. He divided the cosmopolitan *Monticulipora petropolitana* into at least a dozen different genera and several times that number of species, which was too much to be believed. I remember a sarcastic note printed at that time to the effect that he received three small branchlets of a single bryozoan stock from three different men and described all as different species, utterly untrue but characteristic of the venom of some of his critics.

Schuchert urged Ulrich to seek employment in describing and illustrating the chapter on Bryozoa in one of the state surveys

which then were contemplating large works on paleontology. As a result A. H. Worthen, Director of the Illinois Geological Survey, offered Ulrich a contract for the illustration of Volume 8.

This fine lithographic work paid Ulrich well enough to set up housekeeping at 1004 Central Avenue, Newport, an address that became well known to visiting American and European geologists. He married in 1886 Albertina Zuest, a schoolteacher who combined an appreciation of paleontology with a fine regard for good housekeeping neatness and thus made an ideal helpmate for a scientist. I learned to admire her on our European trip in 1922.

Ulrich's fine work on Volume 8 led to his engagement by Dr. N. H. Winchell to describe and illustrate the Ordovician Bryozoa for the Paleontology of Minnesota. Schuchert, who had lost his furniture business through a fire, joined him and lived with him at his Newport home. Ulrich's memoir on the Bryozoa and Schuchert's on the Brachiopoda became fundamental standard works on these two classes of fossils for American paleontologists.

In the latter part of 1888 James Hall, who was collecting material for use in his brachiopod monograph, Volume VIII, Paleontology of New York, came to Cincinnati and, impressed by Schuchert's ability and enthusiasm, as well as his fine brachiopod collection, invited him to return with him to Albany. Schuchert accepted and thus started on his long and successful career in the east.

Ulrich devoted the following three years to the reports on Paleozoic Bryozoa and Sponges for the Illinois Survey and then spent a year on the final report on the Minnesota Bryozoa. While the active geological operations were suspended in Minnesota for 1½ years in 1888-1890, Ulrich was busy in field and office work for the Kentucky Survey, working out the classification and tracing the formations of Mississippian and Pennsylvanian ages in the western part of the state. He worked out reports on the geology of Caldwell and Crittenden counties, which failed of being printed because of the Governor's veto

of the appropriation for geology. In these reports Ulrich recorded the discovery of two recurrences of the lithology and fauna of the celebrated Warsaw or Spergen Hill deposits; the location of a strip of coal bearing land, a mile wide and 8 miles long; the presence of several peridotite dikes, and that the numerous fluorspar, lead and zinc deposits of western Kentucky are true fissure veins and not mere local deposits in eroded cavities as had heretofore been held. His conclusions were later verified by the Federal Survey in Professional Paper 36 (1905).

Ulrich was always fascinated by fossils that had been neglected by others because of the difficulties of their study and the necessity of thin sections. The most important of them were the bryozoans. He continued to study them with the resumption of his work in Minnesota. His 1882-1884 classification of the Paleozoic Bryozoa was elaborated in the 1890 paper, which formed the greater part of Volume 8 of the Illinois Survey, and again in 1895 in the report of the Geological Survey of Minnesota. The work has stood the test of criticism and remained the basis for the study of these puzzling fossils. True to his dedication to the neglected groups, he, at the same time, took up the study of the American Paleozoic sponges, which also required thin sections. The results were also published in collaboration with Oliver Everett, who had collected many fine specimens for Volume 8 of the Illinois Survey. Next followed the Ostracoda, which received attention in several smaller articles in 1891-1892 and the monograph of 1897 on the Paleontology of Minnesota. The ostracodes always remained his pets. I still remember how we two were approaching a railroad station when he noticed some ostracodes in a rock by the roadside, sat down and collected furiously while we missed our train and dinner. Another time on the island of Gotland he saw some ostracodes in an abandoned quarry and forgot all his work intended for the day; but I also remember that when we were once "at sea" about the age of a rock ledge in the woods of New York state he knocked out some ostracodes and determined the age of the rock. True to his fascination for neglected fossils he next took up the Ordovician Lamellibranch-

iata and Gastropoda and published three memoirs in the Minnesota Paleontology and in Volume 7 of the Ohio Geological Survey. These papers, also, have proved of fundamental importance for later work by others, the writer included.

The last of the local amateurs to join the Ulrich association of aspiring collectors was Ray S. Bassler, then a fifteen-year-old high school freshman, who, encouraged by the school principal, went to Newport and timidly called upon the august man and was invited to come as student assistant whenever time from school permitted. Bassler helped Ulrich for eight years. Thus started one of the most fortunate associations that proved wonderfully fertile for science, for Bassler became the world's leading student of fossil bryozoans. A third member of the group was Nickles, who wrote the first systematic account of Cincinnatian stratigraphy and became the bibliographer of the Federal Geological Survey. Later, under the auspices of the Geological Society of America, he published the bibliographies of the geology of the world, exclusive of North America.

Several months were spent for a Newport firm on exploratory work for phosphate, but the approach of the depression at the end of the century brought prospecting in the Central Basin to an end and hard days came for the free lance geologist. Ulrich had to put up his collections as collateral at the banks to keep home and laboratory going. It was fortunate in these dark days that he could sell collections of thin sections of Bryozoa to the British Museum, the University of Munich and other institutions in both Europe and America, and thus could keep going for several years. It required hard work as the sections had to be ground by hand on a sandstone slab. This recalls to the writer that he ground the material for his early papers on an old iron stove lid.

Recognition for the scientist, already 40 years old, was slow in coming in spite of his outstanding work. A break came in 1897, when a collector was needed by the Federal Survey to make another attempt to find fossils in the disputed Ocoee slates of eastern Tennessee. At the suggestion of Dr. C. Willard Hayes he was given a trial at the job. Although he failed, as

had all previous collectors, he impressed Doctor Hayes by his sound work and was shortly afterward invited to assist him in mapping the Columbia, Tennessee, quadrangle. This led to his permanent appointment on the Federal Survey.

Ulrich had now the opportunity to apply his knowledge of fossils to broader studies in correlation, paleogeography and diastrophism and he made the most of it. It became evident with the many folios under preparation that the old time stratigraphic divisions, such as the Trenton limestone, would have to be subdivided before the finer details of structure could be depicted on a map. Ulrich now plunged into problems in stratigraphy and correlation, and true to his thorough nature he had to see for himself. This was the time when he came to New York and the writer took him for six weeks over the Cambrian, Ordovician and Silurian terranes of the State. Ulrich, until his retirement in 1932, undoubtedly saw more Paleozoic geology from the Rockies eastward than any other person. Owing to his incredibly retentive memory he was always able to enter a discussion on any Paleozoic formation and he had buried in his memory an overwhelming amount of facts. I recall, on our trip to Europe together, that one day when sitting on the deck of the steamer looking out at the sea I said casually that I had come to the conclusion that the graptolites (an extinct class of organism of great importance for intercontinental correlation of strata) are related to the bryozoans. Ulrich said: "I have known that for 15 years." I wondered and asked on what grounds. He said: "They have ooecia like bryozoans." I then proposed a joint paper, which was published later. I had observed on splendid material I collected in Oklahoma muscle-scars that convinced me of their relationship to bryozoans. When later I studied the graptolite collections of the U. S. Geological Survey which I had borrowed I found specimens with Ulrich's label pointing to the small sacs attached to the rhabdosomes as "probable ooecia such as bryozoans have"!

Ulrich was now occupied mainly with work on folios. He published the Columbia Folio with C. Willard Hayes in 1903.

There were publications on the western Kentucky lead and zinc district with W. S. Tangier Smith, on copper deposits in the Mississippi Valley with H. Foster Bain, and a number of other folios. The first of these were the Tahlequah, Winslow and Muskogee quadrangles of Oklahoma with Mr. Taff. It was while working on these quadrangles that Ulrich met and hired as assistant, a young student, Rector Mesler, whom he brought with him to Washington and who remained his assistant until Ulrich's death.

There followed the Fayetteville, Ark., folio with George I. Adams, and the Yellville, Ark., Mineral Point, Wisc., and the four Big Horn Mountain folios. On the Wisconsin work he became acquainted with the state geologist Dr. W. O. Hotchkiss (later president of the Rensselaer Polytechnic Institute) and a close friendship sprang up. The two families used to visit each other annually, the men spending their time in looking over the geological field.

Of especial importance was Ulrich's connection with the folios covering the Appalachian Mountain system. This field work, especially in connection with George W. Stose, resulted in the revision and mapping of the Shenandoah limestone and its component units. A whole series of folios, starting with the Mercersburg-Chambersburg and other Pennsylvania folios, was taken up by these authors as far south as the Birmingham, Ala., quadrangle, mapped by Charles Butts. Among these folios are the Pawpaw-Hancock folio of Maryland, the Belmont, Allentown, and Reading folios of Pennsylvania, and the Eureka Springs and Harrison quadrangles of Arkansas. He was persistently busy in those years preparing stratigraphic and paleontologic reports for the folios and monographs of the Geological Survey, and he was, so to say, the final authority on all problems that the mapping geologists of the Survey encountered in their work. I always found him busy handing out advice to others.

None of this important work is reflected in his bibliography, but it led to the publication of his master work: Revision of the Paleozoic Systems (1911). This became my bible on geologic problems and it will be a safe guide for all coming

geologists. In this Revision he explains the principles of stratigraphy and proposes radical changes in the classification. The most important part of the Revision is the proposition and description of two new systems, the Ozarkian and Canadian. He made those systems by taking part of the upper Cambrian and lower Ordovician, claiming important breaks above and below these systems. I know from his attitude that he considered the creation of these two systems as the crowning achievement of his career. With these two systems he advanced into line with Murchison, Sedgwick and Lapworth. I shall always remember how Ulrich, Cushing and I roamed New York to find the breaks in the Little Falls dolomite. It was arduous work and the two friends, Ulrich and Cushing, fought all day long. I went along quietly so that they would not both jump on me. I also remember how unhappy Ulrich was in 1922 in Sweden, when he could not locate the Ozarkian in spite of the help of the friendly Swedish geologists.

Bassler writes: "Doctor Ulrich believed that these new systems were based on criteria similar to those used in separating the well known Cambrian, Silurian, etc. The Ozarkian, he thought, shows the same sequence of diastrophic events and develops as great a thickness of marine deposits as any of the previously established systems. The published records of the Canadian showed this to be equally true for it. Believing that the Ozarkian rocks rest on the top of the typical Upper Cambrian and below any separated as the Canadian, he considered that its 8,000 feet of limestone in the Ozark uplift and the Appalachian Valley certainly represented a sufficient time interval for consideration of it as a system. He acknowledged that the described Ozarkian faunas were too few in species to afford a good argument but the known undescribed species, especially the straight and curved cephalopods and the numerous coiled gastropods absent in the Cambrian faunas and so different from the wealth of graptolites and the first of the coiled cephalopods of the Canadian, were certainly sufficiently diagnostic. Objections to these new systems arose, and today there seems to be a tendency to recognize the Canadian as valid and to abandon the Ozarkian by referring its strata to the Canadian and the Cambrian. Time and more investigations, including particularly the publication of more complete paleontological evidence, will bring the truth."

Ulrich, Foerste and others in recent years published the descriptions of the Ozarkian cephalopods, and Ulrich and Cooper the descriptions of the Ozarkian brachiopods.

On Ulrich's fifty-fourth birthday, February 1, 1911, Rector Duvall Mesler was appointed as Ulrich's personal assistant, a position he held to the end of both their lives. Rec, a student of geology at the University of Arkansas under Professor A. H. Purdue, had been recommended to Ulrich while working in the southwest and Ulrich brought him back with him to Washington. Rec was the ideal assistant for Ulrich. An excellent collector and skilled preparator, he relieved Ulrich of many annoying tasks at the office and also at home. He never married and always lived with the Ulrichs. My wife and I when visiting there always admired and wondered at his devotion. He showed it in a sublime way by passing out of life on the day of Ulrich's burial. His life work was done. He had no selfish ambition for himself, and indeed he did his part in forwarding our science. He showed his friendship and kindness by giving my wife a chance to see Washington and its beautiful surroundings, Ulrich himself having no time to waste on scenery. The latter's mind was so set on work that even in Sweden and Switzerland he refused to spend an hour, much less a day, in seeing and admiring the landscape. He had a one-track mind and a single purpose and interest in life. That may be the underlying cause for the greatness of many a man.

No perplexing problems in the geologic work at the Survey were sidestepped. Such a one was the black shale problem connected with the question of the Devonian or Mississippian age of the Chattanooga formation. The modest paper on the subject published in 1912 gives no inkling of the work spent on the problem in the field and laboratory. It led to a general discussion of the problem by various authors with diverse conclusions.

The next task came when the Maryland Geological Survey delegated Ulrich to prepare the chapters on several groups of organisms, particularly the Ostracoda. The Silurian volume, although intended as a taxonomic study, owing to Ulrich's

thoroughness developed into a stately volume on the stratigraphy as well as generic and specific descriptions of the Ostracoda.

After the completion of this work the Chester controversy, based primarily on the correlation of certain Upper Mississippian formations, attracted Ulrich's attention on account of its bearing on the principles involved. The work necessitated extensive field work with Charles Butts and others in western Kentucky and adjoining states. The results were published between 1916 and 1926.

Ulrich's monthly reports to the Director during this period reveal an amazing amount of work on quadrangles all over the eastern and central United States from Wisconsin and Michigan to Maryland in the east and it is a characteristic of Ulrich that his associates in the field, as Hotchkiss in Wisconsin, Buehler in Missouri and Butts and Stose in the south and east became his close friends and admirers. They all loved him for his knowledge and honesty and frankness of opinion.

Biologists have divided investigators into splitters and lumpers. Ulrich, owing to his keen, discerning mind, was above all a splitter, both in his stratigraphic work and in his descriptive work on fossils, but he could also become a lumper when he was looking for general principles and conclusions.

Of the continuous stream of papers that flowed from his pen, usually in collaboration with young men, we will mention papers on the Revision of the Paleozoic Bryozoa (with Bassler) in 1904-1905; also with Bassler, New American Paleozoic Ostracoda in 1908; on cystids and crinoids between 1921-1928; on conodonts (with Bassler) in 1926; with Resser, several trilobite articles in 1930; and (also with Bassler) a monograph on Cambrian bivalved crustaceans in 1931 and with Cooper on Ozarkian and Canadian Brachiopoda in 1938. Ulrich was usually at work on several papers. Once when I was in his office he spread out 17 papers before me and said: "Ruedemann, you will have to come here and help me with them!" When he took up an investigation, he soon discovered that it led to others on all sides.

Some of his papers were not published, as Ulrich would not

compromise with the truth as he saw it. Such a paper concerned the Richmond question. The problem was whether this formation was to be mapped as uppermost Ordovician or lowest Silurian. The writer went out with him into the field when he was seeking the solution of the problem in the western New York Silurian, especially the Medina formation.

In April 1931, at the annual dinner of the National Academy of Sciences, when the writer had the honor and great pleasure of handing Dr. Ulrich the Mary Clark Thompson Medal for his outstanding contributions to geology and paleontology, he pointed out that Ulrich used his immense knowledge first in continent-wide distinctions and correlations of Paleozoic formations and finally in intercontinental comparisons, all this with decisive and undoubtedly lasting results; further, that much of our Paleozoic paleogeography is based on his work; that he recognized that the Paleozoic epicontinental seas of this continent were a multitude of small basins and troughs rather than the wide inundations before assumed, and, finally, that we have to thank him for determining that the evolution of the faunas did not take place in these basins but in the ocean along the edge of the continent.

Doctor Ulrich always claimed that the American marine succession is the most complete and best determined and therefore the best fitted for recognition as the world standard in classifying Paleozoic events. I had much fun on our trip in Europe in 1922 listening to his strenuous arguments with foreign geologists about this problem. He would always bring it up on the first occasion offered; it almost seemed to me that it was one of the principal purposes of his trip to spread this gospel among the "backward" European geologists. He made six trips to Europe between 1922 and 1931 to check his results in America with the classic outcrops abroad.

Ulrich resumed the European field trips in the summer of 1925, when, in company with his friends Prof. Richard M. Field, the late Milton Fullé and the late Charles E. Resser, stratigraphic problems were studied in Great Britain, Norway and Bohemia. The next year he returned to Europe for the Inter-

national Geological Congress in Madrid and further study of European stratigraphy. Further trips mostly to the British Isles followed in 1927, 1929 and 1931.

Finally, in the early summer of 1933 the Washington papers copied a London announcement that Miss Lydia Sennhauser and Dr. E. O. Ulrich had been married there on June 20, with Dr. C. J. Stubblefield of the Geological Survey of Great Britain officiating as best man. My wife and I can heartily verify by our own observation Dr. Bassler's statement as to their happiness. Bassler writes in his Memorial: "Knowing Mrs. Ulrich and her genial, sympathetic nature, his friends realized that the rest of Uncle Ed's life would be free from any cares and responsibilities which might restrict him in carrying on his studies." Lydia told me on the steamer in 1922 about her arduous work as nurse in Davos, a famous resort for consumptives in Switzerland. There is no doubt in my mind that she proved a godsend to the aged scientist in his declining years. She is probably very proud to have been the wife of such a great scientist.

Anyone who visited Ulrich in his office could not fail to observe how he was at all times ready to help out his colleagues from his great store of information. They would come into his office without hesitation and get his advice on various geologic problems. He ate his simple lunch in his office, and even that time would be used for discussion.

Dr. Ulrich was an original fellow of the Geological Society of America and also of the Paleontological Society. Well earned honors came to him from America and foreign countries. Dr. Bassler lists them as follows: "He served as president both of the Paleontological Society and the Washington Geological Society. He was a member of the Washington Academy of Sciences, the National Academy of Sciences (1917), the Academy of Natural Sciences of Philadelphia (1932), of Sigma Xi (1936), Foreign Correspondent of the Geological Society of London (1927), the Geological Society of Stockholm (1927) and of the Senckenberg Institution (1933). His name was starred as early as 1910 in American Men of Science. Various

honors in recognition of his researches came to him. In 1931 the National Academy of Sciences awarded him the Mary Clark Thompson Medal, as noted above, and in 1932 the Geological Society of America presented him with the Penrose Medal for distinguished attainment and outstanding contributions to the science of geology."

Owing to his strong physique, he held up well both physically and mentally until shortly before the end. In spite of the crowded conditions in Washington, he would insist on the long bus ride to the Museum every day until several months before the end. The latter came early in the morning of February 22, 1944, when he passed into a gentle sleep from which he did not awake.

Ulrich's fame is established forever among geologists, and a host of younger generations of geologists will remember him for many years to come with love and admiration.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

Amer. Geol. = American Geologist.
Amer. Jour. Sci. = American Journal of Science.
Cin. Soc. Nat. Hist. Journ. = Cincinnati Society of Natural History, Journal.
Geol. Soc. Amer. Bull. = Geological Society of America, Bulletin.
Geol. Soc. Amer. Sp. Pa. = Geological Society of America, Special Papers.
Ill. Geol. Sur. = Illinois Geological Survey.
Md. Geol. Sur. = Maryland Geological Survey.
Minn. Geol. Nat. Hist. Sur. = Minnesota Geological and Natural History Survey.
N. Y. State Mus. Bull. = New York State Museum, Bulletin.
Ohio Geol. Sur. = Ohio Geological Survey.
Okla. Geol. Sur. Bull. = Oklahoma Geological Survey, Bulletin.
Smith. Inst. Ann. Rep. = Smithsonian Institution, Annual Report.
Smith. Misc. Coll. = Smithsonian Miscellaneous Collections.
U. S. Geol. Sur. Bull. = United States Geological Survey, Bulletin.
U. S. Nat. Mus. Proc. = United States National Museum, Proceedings.
Wash. Acad. Sci. Jour. = Washington Academy of Sciences, Journal.

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L H Baekeland

NATIONAL ACADEMY OF SCIENCES

OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XXIV—EIGHTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

LEO HENDRIK BAEKELAND

1863–1944

BY

CHARLES F. KETTERING

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1946

LEO HENDRIK BAEKELAND

1863–1944

BY CHARLES F. KETTERING

Leo Hendrik Baekeland was born in Belgium, in the Flemish city of Ghent, on November 14, 1863. He was a son of Charles and Rosalie (Merchie) Baekeland, a Belgian family of moderate circumstances. Entering school at the age of 5, he passed through the elementary schools and the Atheneum, a government high school. When old enough he entered the Ghent Municipal Technical School, where he attended evening classes in chemistry, physics, mechanics, and economics, and won a medal in each of the four subjects.

Young Baekeland was such a promising student that the City of Ghent awarded him a scholarship in the University of Ghent, and he entered that university in 1880 at the age of 17. He was the youngest member of his class, but the most brilliant. In 1882 he graduated from the university as a Bachelor of Science. In two years more, or in 1884 at the age of 21, he gained the degree of Doctor of Science, maxima cum laude. Furthermore, with the aid of the City Scholarship he had received, and by teaching and serving also as a lecture assistant, he supported himself while in the university. Baekeland was inspired to do this, and so to relieve his parents of his support, he said later, by having early heard the story of Benjamin Franklin and having learned from it that a boy in humble circumstances could make his way altogether by his own efforts.

In the university Baekeland studied the natural sciences and specialized in chemistry. A boyhood interest in photography was one of the things which interested him in chemistry and which attracted him to it as a major. In some of his early experiments on photography young Baekeland had needed silver nitrate. He had no money to buy it, but he did have a watch with a silver chain which his father had given him. So he took the chain off his prized watch and dissolved it in nitric acid. There was copper in that solution too, but young Baekeland

worked out, as one of his earliest chemical operations, a plan for removing the copper from the silver solution.

It was at the University of Ghent that Kekule had taught and it was there in 1865 that he announced his classical theory of the structure of benzene. Some years before Baekeland's time, however, Kekule had left Ghent for Bonn, and his chief assistant, Theodore Swarts, had taken his place as senior professor of chemistry. And it was under Professor Swarts that Baekeland studied.

In 1887 Baekeland was appointed professor of chemistry and physics at the Government Higher Normal School of Science in Bruges. He did not stay long at Bruges, though, for his own University of Ghent soon offered him a post as assistant professor. He gladly accepted that offer, and soon afterwards, in 1889, he was promoted to associate professor.

One of the reasons for Baekeland's desire to return to Ghent was that he had fallen in love there with Celine Swarts, the charming daughter of his professor of chemistry, Theodore Swarts, and his wife Nina (Plateau) Swarts. The two young people were married on August 8, 1889, and the marriage proved a particularly happy one. In later years at a gathering to do him honor, Baekeland said, "You have talked here tonight about my many discoveries. But you haven't mentioned my greatest discovery—a discovery I made when I was still a student. That great discovery was a woman who is here with us tonight—my wife." Aside from the contributions she made to the success of her husband, which were many, Mrs. Baekeland became famous in her own right, notably as a painter in oils.

In a competition among the alumni of the four Belgian universities who had graduated during the preceding three years Baekeland won in 1887 first prize in chemistry. That gave him the title of laureate in chemistry, a gold medal, and a traveling fellowship. And so, in 1889 when he was 26 years old, Baekeland visited University College, London, Oxford University, and the University of Edinburgh. After that, accompanied by his young wife, Baekeland sailed to the United

States, where he planned to spend some time in continuing his researches, particularly in the chemistry of photography. Baekeland's native city of Ghent was a center for the manufacture of photographic dry plates, an industry started there in the 1880's by Van Monkhoven. Monkhoven had taken an interest in young Baekeland, and with his encouragement Baekeland began early to experiment with photography and with the chemistry of its processes, and to try to extend the knowledge in the field.

Upon his arrival in New York, Baekeland made the acquaintance of Richard A. Anthony of E. and H. T. Anthony and Company, manufacturers of photographic materials. And through Richard Anthony he was introduced to C. F. Chandler, professor of chemistry at Columbia University, who was a chemical consultant to the Anthony Company and who also, as an enthusiastic amateur photographer, was then editing the *Photographic Bulletin* published by that company. Professor Chandler, being impressed by the capabilities of Baekeland, persuaded him to remain in the United States and to apply his talents to the solution of chemical problems in industry. Accepting the advice of Prof. Chandler, Baekeland cabled his resignation from the faculty of the University of Ghent. The Minister of Education of Belgium, in accepting Baekeland's resignation, authorized him to retain as an honorary title that of Associate Professor at the University of Ghent.

In 1891 the E. and H. T. Anthony and Company offered Baekeland an excellent position as chemist in their factory, which offer he accepted. It was the Anthony Company, makers of photographic dry plates and bromide paper, which afterwards joined with the Scoville Company to form the Ansco Company.

In 1893, however, two years after he had begun to work for the Anthony people, Baekeland resigned his position to become an independent consulting and research chemist. Chiefly he planned to devote his time to developing a number of chemical processes which he had devised; and, as he said later, he made the mistake of scattering his attention on too many subjects at the same time. But at that stage a serious illness came upon

him. And that brought him to a decision which he expressed in these words:

> While I was hovering 'twixt life and death, with all my cash gone and the uncomfortable sentiment of rapidly increasing debts, I had abundant time for sober reflection. It then dawned upon me that instead of keeping too many irons in the fire, I should concentrate my attention upon one single thing which would give me the best chance for the quickest possible results.

The one thing which Baekeland decided to do after his recovery was to found in Yonkers, with the financial and managerial assistance of Leonard Jacobi, the Nepera Chemical Company, and to begin to manufacture on a small scale photographic papers and chemicals. One of those papers was named "Velox," and it was destined later on to become very widely used indeed and to yield a financial return which set Baekeland free and put him in position to make other outstanding discoveries. Baekeland had begun ten years before, while still a student at Ghent, the research which culminated in Velox paper; but he had not previously appreciated its commercial importance. What he had found out was that, in preparing a silver chloride emulsion, the customary "ripening" process and the subsequent washing step had a disastrous effect upon the emulsion in respect to the tone and the general gradation of the image, especially in the shadows. By preparing silver chloride in a special colloidal condition and omitting altogether the customary washing step, Baekeland made a distinctly superior photographic paper, and one which printed much faster than older papers. Prints were made by exposing for a short time to artificial light and then developed at a little distance from the same light, which was a distinct improvement over the slow and unreliable method of "sun printing" then in use. Because of the different processing Velox paper required, however, it did not meet with general favor at first; and for that reason, coupled with the depression of 1893, the new company passed through some very difficult times. Baekeland spoke once of that period as "several years of hard work, with

never a single day of rest, and ever wondering whether I would pull through or not." He said further:

> I had been too optimistic in believing that the photographers were ready to abandon the old slow processes of making photographic prints. I had to find out then how difficult it is to teach anything new to people after once they got use to older methods. . . . Even my best friends tried to dissuade me from continuing my stubborn efforts. I had also not foreseen manufacturing difficulties, but I gradually managed to overcome them.

Largely through use by amateurs—who, as Baekeland said, "began to give themselves the trouble of reading and following our printed directions"—the sales of the new paper gradually grew, and by 1899 the business had become so successful that the Eastman Kodak Company bought out the interests of Baekeland and Jacobi on very liberal terms.

It was while he was manufacturing photographic papers in the 1890's that Baekeland became a pioneer in air conditioning as an aid to chemical processing. He found that atmospheric conditions, particularly the moisture content of the air, were responsible for large variations in the photographic printing papers produced. Up to that time, only refrigeration had been used for hardening coatings on papers by a chilling process. But, as such chilling had the bad effect of making the coating brittle, Baekeland worked out and installed a system for removing moisture from the air by putting the air through a refrigeration unit and subsequently warming it to the proper temperature by passing it over steam coils before it entered the coating room. This gave rapid drying of the emulsion without the development of brittleness. Being troubled, however, in the winter time with the development of static electricity on the paper, Baekeland installed also a system in which silver chains were trailed over the paper on the coating machines to carry off the charge through the frame. And, in a paper presented in 1903, he said, "In photographic paper factories hygrometers and electroscopes should be consulted as often as the thermometer."

After the sale of his photographic paper interests to Eastman, Baekeland purchased as a home for his family and a place where he could continue his experiments the estate in North Yonkers known as "Snug Rock." Situated high above the Hudson, it looked across to the Palisades on the west bank. Of his situation at that time Baekeland said the following:

> Thus at thirty-five I found myself in comfortable financial circumstances, a free man, ready to devote myself again to my favorite studies. Then truly began the very happiest period of my life. I improvised one of the buildings at my residence in Yonkers into a modest but conveniently equipped laboratory. Henceforth I was able to work at various problems of my own free choice. In this way I enjoyed for several years that great blessing, the luxury of not being interrupted in one's favorite work.

From time to time Baekeland employed a number of assistants in his work there, but the greatest of his helpers was his wife, who assumed the responsibility of keeping his records, and whom he consulted on many of his problems and transactions.

At this time Baekeland became interested in electrochemistry. This was because he saw that electrochemistry, which in his student days had been limited to the electro-deposition of a few metals from aqueous solution, had since that time become an important branch of chemical industry. It was then being put to such uses as separating aluminum from bauxite, producing carborundum and graphite, making calcium carbide, manufacturing sodium, and simplifying the preparation of important compounds of that metal, such as sodium cyanide. Because of his interest in these developments, Baekeland decided in 1900 to visit Germany for what he called a "refresher" in the science of electrochemistry. He spent a winter there in the electrochemical laboratory of the Technological Institute of Charlottenburg brushing up on his knowledge of the subject. And when he returned to Yonkers he fitted his laboratory with electrochemical equipment for further study.

It was about that time that Clinton P. Townsend invented his electrolytic cell for producing caustic soda and chlorine from

salt. And Baekeland was asked by Elon H. Hooker to undertake an investigation of the Townsend electrolytic cell, preliminary to its application on an industrial scale. This Baekeland did in company with the inventor and several other persons skilled in electrolysis. An important one of Baekeland's contributions to the Townsend cell at that time was an improved diaphragm of much greater durability than had been available before. That work led to two of the earliest of the many patents which Baekeland took out, and to the formation in 1903 of the Hooker Electrochemical Company and the erection at Niagara Falls of one of the largest electrochemical plants in the world. For several years afterwards Baekeland continued to be connected with that company in a consulting capacity.

In his work on the Townsend cell which preceded the building of the Hooker plant at Niagara Falls, Baekeland constructed two full-size electrolytic cells embodying the improvements made by his group; and these he operated under varying conditions, day and night, for months. With the further knowledge thus gained, the specifications for the full-sized plant were drawn. But even that plant was not built in full at first, only the smallest section that could be operated. Thus, by this careful step-wise procedure, involving an expenditure of only $300,000, as Baekeland said later, blunders that might have cost millions were prevented. And that experience gave rise to a maxim of Baekeland's which has been widely quoted: "Commit your blunders on a small scale and make your profits on a large scale."

Baekeland then turned his attention to the subject upon which his greatest fame rests—the research which pioneered the important plastics industry. By finding how to direct the action of formaldehyde upon phenols in proper channels, he gave to the world an important new material which was named and trade-marked "Bakelite." The condensation of aldehydes with the phenols was not a new reaction at all. It had been known for twenty years, or ever since the work of Adolph Bayer in 1872. But the condensation of formaldehyde with phenol does not of itself give bakelite. Even when the reaction yielded a

resin, it was one that had no special utility. Only under the very special conditions established by the long researches of Baekeland is a product of the amber-like and highly resistant properties of bakelite produced. Nor was Baekeland the first investigator who had tried to obtain that result. Years afterwards Baekeland said of those earlier workers, "They *should* have succeeded, but they wouldn't." What he did not say was that it was only after five years of the most intensive effort, and after many failures and disappointments, that he himself succeeded.

But, as a result of a long and systematic investigation, in which he tried to study all factors of the reaction between formaldehyde and phenol, Baekeland found that he could dissect the reaction or separate it into different steps. He found that pressure was valuable in controlling the reaction, and that by the presence of ammonia or other base he could spread the reaction out over a longer period and so could stop it at any stage he wished by cooling. He found that he could thus control the reaction in steps, and use was made of that fortunate circumstance in furnishing the new product, bakelite, to those who had use for it.

In respect to making the new material available commercially to those who had need of it, Baekeland said this:

I firmly intended to escape the recurrence of business occupations, as in my Velox days. So I planned, instead of manufacturing myself, to grant licenses to established manufacturing concerns, especially experienced in plastics. But I soon was confronted with a repetition of my former experience with Velox: that it was very difficult to teach new methods to men who had acquired routine in older processes. The preparation of the new resinoid and its molding compositions, which to me seemed very simple, appeared either very difficult or needlessly complicated to others. Reluctantly I had to start manufacturing the raw materials in a sufficiently advanced stage so that the users had only to complete the operation of molding and polymerization.

The bakelite resin, as thus produced for distribution to the trade, would soften on heating and it could be dissolved; but,

on further heating, it set into a permanently hard and insoluble substance, which was strong, which had excellent electrical insulating properties, and which was resistant to heat and to many chemicals. The material thus found many important uses, chiefly at first in replacing hard rubber and amber in electricity and industrial arts at places where those materials were not satisfactory. In my own case, for instance, as a maker of ignition systems for automobiles at that time, bakelite served a very useful purpose indeed in respect to such vital parts as distributor heads. The hard rubber, which, before bakelite became available, had had to be used for molding distributor heads, gave trouble whenever conditions were such that it got hot. But distributor heads molded out of bakelite were strong and altogether free from troubles due to temperature.

The early manufacture in connection with the commercialization of bakelite was done in Yonkers. But in 1910 a company called the General Bakelite Company (later the Bakelite Corporation) was organized to manufacture and distribute the raw materials for making bakelite parts, and a factory was then established at Perth Amboy, N. J. Baekeland served as president and moving spirit of that company from the time of its organization until in 1939 it was merged with the Union Carbide and Carbon Corporation. Nevertheless, by intelligent organization and by careful selection of associates, Baekeland was able to keep free enough from routine and business entanglements so that he could maintain his interest in research, and could still devote some time to it, as well as to the numerous scientific, patriotic, and educational calls which were made upon him.

Baekeland published the results of his experiments in full in the many scientific papers of which he was author. In all, he published about 75 papers, letters, and addresses. It has been suggested—by Wallace P. Cohoe—that, in considering the magnitude of the work reported in Baekeland's scientific papers, they should be weighed rather than counted. And it surely is true that, by the customary standards in publishing the results of scientific research, some of Baekeland's papers could well

have been divided to give more titles; but that was a thing to which he was indifferent.

Except in the case of Velox, Baekeland also took out patents to protect his discoveries—more than a hundred patents in all, including domestic and foreign. He was a believer in the worth of the patent system and much interested in proper patent procedure. He was a member, and for one year chairman, of the Committee on Patents of the National Research Council, and a number of his published papers relate to what he believed were needed modifications of the United States Patent System. On his own experience with patents, Baekeland said this:

> One of the evidences of a successful patent is infringement. So I had to go through the experience of almost every successful inventor of defending my rights before the courts. Fortunately, I won every case. Furthermore, I was lucky enough to find among my former rivals many of the excellent men whom I count today as my dearest friends and most distinguished collaborators in our corporation.

Having begun his career as a teacher of chemistry, Baekeland had a lifelong interest in education. He had a gift of talking on the many subjects which interested him in a manner that held the attention of all who heard him. He was always ready to give others, and young people in particular, the benefit of his knowledge and experience. Respecting his own education, Baekeland said this in his Perkin Medal Address in 1916:

> I feel very grateful for the excellent opportunities of education I had at the University of Ghent. I should state, however, that my real intense education only began after I had left the university, as soon as I became confronted with the big problems and responsibilities of practical life; this education I received mainly in the United States, where for twenty-seven years I was thrown in contact with so many varied subjects. I hope to remain until I die a post graduate student at that greater school of practical life, which has no fixed curriculum and where no academic degrees are conferred, but where wrong petty theories are best cured by hard knocks.

In 1917 Baekeland accepted an honorary professorship of chemical engineering at Columbia University. The value of

Baekeland's service to that university, where in early life he had received vital inspiration from Professor Chandler, was once expressed by his colleagues there in these words:

> For more than a quarter of a century, his wise counsel and brilliant lectures, which were enrichened by a vast scientific knowledge and an almost limitless industrial experience, brought to the university a high quality of inspiring instruction and sound research enthusiasm that had much to do with giving Columbia the high reputation it has in chemical and chemical engineering education and research throughout the world.

Baekeland was a member of the important scientific societies in his field, both in the United States and abroad, and he was elected to the National Academy of Sciences. No matter how busy he might be, he found time to attend scientific meetings, and also to take part in them. He was most active in the societies devoted to chemistry, of course, and he served as an officer of a number of them. He was president of the Electrochemical Society in 1909; of the American Institute of Chemical Engineers in 1912; and of the American Chemical Society in 1924; as well as of the Chemists' Club in New York (of which he was one of the founders) in 1904. In 1906, only seventeen years after coming to the United States, Baekeland was chosen to represent the chemists of America at the Jubilee of the Foundation of the Coal-Tar Color Industry by Sir William Perkin. Also, in 1909 he was U. S. delegate to the International Congress of Chemistry, and he was president of the Section on Plastics when in 1912 the Congress met in the United States. How much his association with his fellow chemists meant to Baekeland was expressed in the conclusion of his Perkin Medal Address, as follows:

> My friends, chemists of America, how can I let pass an occasion like this without reminding you of what *you* did for me?
>
> Twenty-seven years ago I came here as a stranger among you and now I feel so much as one of you that sometimes I wonder that there was ever a time when we did not work and play together.
>
> When I was young and poor and unknown you never hesi-

tated to extend to me the cordial hand of welcome, you never missed an opportunity to show me your friendliness, to help me by advice or otherwise. Much of what I have used in my work I learned from you at the meetings of our chemical societies, or in the brotherly surroundings of our Chemists' Club.

You—your friendship, your generosity, your good-natured modesty, your example, inspired me in my work.

What Baekeland did not say in the above was that, by the very active part he took in chemical affairs and by the full presentations of the results of his scientific and industrial researches, he contributed to the profession as much as he himself received, or even more.

In his personal life Baekeland liked simplicity. He rose early and retired early. He worked hard and made heavy demands on his physical and mental energy. He was usually at work before other members of his staff. He was an excellent conversationalist and greatly enjoyed associating with congenial people. Possibly out of his life-long interest in photography, he had a great interest in motion pictures, and he often took time out in afternoons to see the new motion pictures being shown in New York.

One of Baekeland's chief hobbies was motoring. He began driving a car in the late 1890's when motoring was little more than a sport. He was one of the first to take long motor trips, having in 1906 gone with his wife and two children on an automobile tour through Europe. He afterwards wrote a long account of that extensive trip, giving the results of his experiences over there as a pioneer motor tourist, as well as interesting and humorous accounts of what happened to the family and what they saw. That story, illustrated with many of the beautiful photographs which Baekeland took on the tour, was published serially in 1907 in the pioneer motor magazine, *Horseless Age*. Later the several chapters were assembled by the publishers of *Horseless Age* and issued as a book, "A Family Tour Through Europe" by Dr. L. H. Baekeland.

Baekeland was also an enthusiastic yachtsman. His first

boat, purchased in 1899, was a gasoline launch in which gasoline served both as the expansive fluid in the boiler to drive a reciprocating engine, and as the means of firing the boiler! In that launch Baekeland, accompanied by Maximilian Toch, went on a cruise from Yonkers up the Hudson and connecting waters to the St. Lawrence and back. And, in spite of the seemingly hazardous nature of the outfit, they returned home safely. In 1915 Baekeland purchased a 70-foot yacht, which he christened the "Ion." It was in some respects of unusual design, constructed after Baekeland's own ideas, and was equipped with a diesel engine as auxiliary power. In the "Ion" Baekeland sometimes sailed in late summer from his home on the Hudson down to Florida and spent part of the winter months in that area, cruising among the islands there, fishing and exploring. Later he purchased an estate at Coconut Grove, Florida, where he lived most of the winter.

Life on his Florida estate gave Baekeland the opportunity of pursuing another of his hobbies, cultivating such rare tropical fruits and flowers as would grow in southern Florida. In this endeavor he was greatly assisted by a neighbor of his at Coconut Grove, the noted botanist David Fairchild. Baekeland used to delight to send to his friends at the North rare tropical fruits from his garden.

In his family Baekeland was particularly fortunate and happy. Mention has already been made of Mrs. Baekeland, a woman who was skilled in music, gifted as a painter, a charming hostess, as well as one who assisted her husband mightily in all his endeavors. So highly did Baekeland value the assistance and inspiration of his wife that he once told Mrs. Wallace P. Cohoe that he never would have amounted to anything but for her help. The Baekelands had two children: a son, George W., now vice president of the Bakelite Corporation; and a daughter, Mrs. Nina Baekeland Wyman.

Baekeland was a long-time member of the U. S. Naval Consulting Board; member of the U. S. Nitrate Supply Commission, 1917; chairman of the Committee on Patents, National Research Council, 1917; a trustee of the Institute of Inter-

national Education for many years; and a member of the advisory board, Chemistry Division, U. S. Department of Commerce, for some years beginning in 1925. Of honors and distinctions he received such a large number that a special list of them is appended to this memoir.

During World War I, when my laboratory was collaborating with the U. S. Bureau of Mines in an effort to secure a better (more nearly knock-free) aviation gasoline, Baekeland was in touch with the endeavor in his capacity as a member of the Naval Consulting Board. When, after an extensive program of engine tests of hydrocarbons of various types, our men proposed to manufacture cyclohexane as the basis of a better airplane fuel, Baekeland advised against the attempt, as he thought it impractical. To emphasize his belief, he offered to give the group a wooden medal if they could make a pint of cyclohexane.

The effort to produce cyclohexane by the catalytic hydrogenation of benzene was undertaken nevertheless. And, after an intensive research, the effort turned out to be much more feasible and successful than Baekeland, or we ourselves, had thought. And so, knowing that Baekeland's sportsmanship resided on a high plane, we ceremoniously presented to him from the first batch of cyclohexane produced a liter bottle of it in a plush-lined mahogany box. Along with the sample of cyclohexane, we gave him a suggested design, implying CATalysis, for the wooden medal he had promised to present the group. The outcome was so pleasing to Baekeland that that bottle of cyclohexane became one of his prized possessions, and he kept it on his desk for a long time afterwards. That he should have been so pleased when a prediction of his turned out to be wrong was because his long experience in industrial chemistry and consultation work had taught him, as he once expressed it, "to bow humbly before the facts, even if they did not seem to agree with my favorite theories." All his own successes in research, Baekeland said also, had had their origin in divergences between facts observed in experiment and currently accepted theory.

Leo Hendrik Baekeland came to the end of his long, eventful, and highly useful life on February 23, 1944, at the age of eighty-one. As a boy he had wanted to follow the sea. But, when he chanced to hear a lecture in chemistry, the subject fascinated him so greatly that his search for adventure was shifted into that field instead. And, fortunately, the field of chemistry turned out to be one full of high adventure for him. It was fortunate also that, by virtue of Baekeland's distinctive genius and his hard work, his explorations there yielded discoveries of the highest importance to the world.

HONORS CONFERRED ON LEO HENDRIK BAEKELAND

Doctor of Chemistry, University of Pittsburgh (1916)
Doctor of Science, Columbia University (1929)
Doctor of Applied Science, University of Brussels (1934)
Doctor of Laws, University of Edinburgh (1937)
Member, National Academy of Sciences
Honorary Member, Royal Society of Edinburgh
Life Member, American Philosophical Society
Life Member, American Association for the Advancement of Science
Life Member, Franklin Institute
Life Member, Royal Society of Arts (London)
Life Member, Société Chimique de France
Honorary professor, Columbia University
Honorary Member, American Institute of Chemists
Honorary Member, Electrochemical Society (president, 1909)
Honorary Member, Société Belge des Électriciens
Honorary Member, Société de Chimie Industrielle (Paris)
Vice president, Society of Chemical Industry, London (1905)
President, American Institute of Chemical Engineers (1912)
President, American Chemical Society (1924)
Nichols Medal, New York Section American Chemical Society (1909)
John Scott Medal, Franklin Institute (1910)
Willard Gibbs Medal, Chicago Section American Chemical Society (1913)
Chandler Medal (first award), Columbia University (1914)
Perkin Medal, Society Chemical Industry (1916)
Messel Medal, Society Chemical Industry, London (1938)
Franklin Medal, Franklin Institute (1940)
Grand Prize, Panama-Pacific Exposition (1915)
Pioneer Trophy, Chemical Foundation (1936)
Scroll of Honor, National Institute of Immigrant Welfare (1937)
Sigma Xi
Phi Lambda Upsilon
Tau Beta Pi
Officer of the Legion of Honor (France)
Officer of the Order of the Crown (Belgium)
Commander of the Order of Leopold (Belgium)

KEY TO ABBREVIATIONS USED IN THE BIBLIOGRAPHY FOLLOWING

Congr. intern. quim. pura aplicada = Report of the 9th International Congress of Pure and Applied Chemistry
Electrochem. Met. Ind. = Electrochemical and Metallurgical Industry
Ind. Eng. Chem. = Industrial and Engineering Chemistry. (Formerly Journal of Industrial and Engineering Chemistry.)
J. Am. Chem. Soc. = Journal of the American Chemical Society
J. Chem. Education = Journal of Chemical Education
J. Franklin Inst. = Journal of the Franklin Institute
J. Soc. Chem. Ind. = Journal of the Society of Chemical Industry
Trans. Am. Electrochem. Soc. = Transactions of the American Electrochemical Society
Zeit. angew. Chem. = Zeitschrift fur angewandte Chemie

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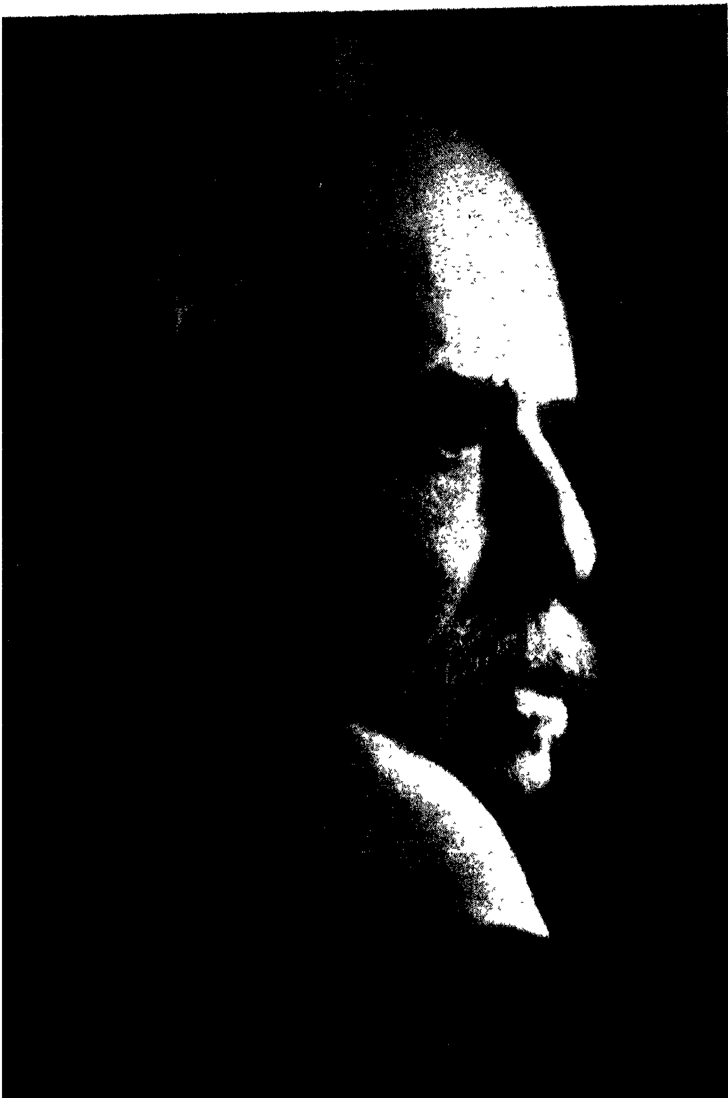

Franz Boas.

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XXIV—NINTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

FRANZ BOAS

1858–1942

BY

ROBERT H. LOWIE

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1947

FRANZ BOAS[1]

1858-1942

BY ROBERT H. LOWIE

Franz Boas, for many years the undisputed dean of American anthropologists, was born in Minden, Westphalia on July 9, 1858. The son of educated parents in easy circumstances, he enjoyed standard preparatory instruction; and to the high ethical teaching imbibed in the household he referred feelingly in an open letter to President Von Hindenburg (March 27, 1933).

Entering the University of Heidelberg in 1877, he later shifted to Bonn and ultimately to Kiel, where he took his Ph.D. in 1881. Though his major interests then lay in physics and geography—his dissertation dealt with the recognition of the color of water—, his principal professor, Theobald Fischer, directed him also towards the historical and ethnographic aspects of geography. Through the mathematical training acquired during his university days Boas was subsequently able to follow, critically and constructively, the rise of biometrics and its anthropological applications. But he did not narrowly specialize. For example, he read Gustav Theodor Fechner, including the delightfully humorous *Vier Paradoxa.* From his son-in-law, Dr. Cecil Yampolsky, I learn that Boas's letters of this period have been carefully preserved and that they reveal the nascent investigator's ardor for research. Publication of the correspondence would be a great boon, for it is likely to reveal intimate glimpses of the writer's personality, such as are all too rarely vouchsafed by his monographs and books.

[1] The American Anthropological Association has issued an obituary memoir (Memoir 61, 1943), which contains a complete bibliography and articles on Boas as a man (A. L. Kroeber), an ethnologist (Ruth Benedict), a linguist (M. B. Emeneau), a physical anthropologist (Melville J. Herskovits), a folklorist (Gladys A. Reichard), and an archeologist (J. Alden Mason). I have drawn upon this publication, especially on Kroeber's biographical essay.

Two years after the doctorate came the crucial expedition to Baffinland, ostensibly in the interests of geographical exploration, but ushering in a new era in Boas's life and in the history of Eskimo ethnography. Homeward bound, Boas paid his first visit to the United States and to New York. On his return to Germany he attached himself as an assistant to the Königliches Museum für Völkerkunde in Berlin, the institution founded and headed by Adolf Bastian; and in 1886 he received permission to lecture at the University as a Docent. Doubtless he regularly attended the Berlin Society for Anthropology, Ethnology, and Prehistory, meeting its ruling spirit, the great pathologist Rudolf Virchow, to whom years later Boas paid a glowing obituary tribute (*Science,* n. s., 16: 441-445, 1902). It would be most interesting to know how these congenial intellects interacted, but nothing specific on their relationship is known. The obvious similarities between the two men have been repeatedly noted—their keen, analytical powers, their exceptional capacities for varied work, their independence, moral courage, and alert social consciousness. Similarly, it would be worth knowing to what extent Bastian influenced the younger man. Conceivably Boas's insistence on definite proof of cultural diffusion goes back to this source, but it is quite as plausibly explained in terms of Boas's own mentality. What personal intercourse with the older man doubtless did provide was an intimate knowledge of Bastian's theoretical views, often veiled for the mere reader by the most crabbed of styles.

The expedition to Baffinland yielded a number of papers, both popular and technical, on the region, the ethnographic publications culminating in the monograph on "The Central Eskimo" (6th Annual Report, Bureau of American Ethnology, Washington, 1888). In the meantime Boas found a new realm to conquer. A party of Bella Coola Indians exhibited in Berlin and the ample collections of the Museum stimulated an interest in northwestern North America. Boas pumped the natives for linguistic information, published the data secured,

and in 1886 himself set forth for the coast of British Columbia. Thus started a notable research programme that occupied him literally until his death.

Returning to New York in 1887, Boas accepted a position as Assistant Editor of *Science* and married Marie Krackowizer, the daughter of an Austrian physician and Forty-eighter who had gained distinction in America both as a medical man and a political reformer. Henceforth the United States became Boas's home.

The British Association for the Advancement of Science had created a committee for the study of the tribes of British Columbia. From 1888 on, during Edward B. Tylor's chairmanship, Boas repeatedly revisited the Northwest coast under this body's auspices. His early reports bear witness to the range of his interests, which took in not only ethnography, but also linguistics and somatology. Sometime during these years Boas visited Tylor and Francis Galton in England, men for whom he retained a profound respect, which *more suo* did not preclude critical dissent. Here again it would be instructive to learn more about the measure of their direct influence. Tylor's famous paper on the application of statistics to sociological problems (1889) certainly impressed Boas; for a while, he told me, it seemed as though everything could be solved by the methods there outlined. Galton he regarded as the true father of biometrics, for which Karl Pearson had furnished the technical apparatus. He recognized, of course, Pearson's exceptional ability and once tried to visit him in England; but Pearson, though he had referred very cordially to Boas in the second edition of *The Grammar of Science,* for some reason declined to see him.

In 1888 Boas accepted a docentship at Clark University, remaining there until 1892, when he had his and America's first anthropological Ph.D. student, A. F. Chamberlain. He left Clark to become F. W. Putnam's chief assistant at the anthropological exhibits of the Chicago World's Fair, the core of the subsequent Field (Columbian) Museum. At this new

institution he served as curator of anthropology, but was sup seded by Wm. H. Holmes. A year or two later he accep an assistant curatorship under Putnam at the Ameri Museum of Natural History, a position soon combined w a lecturership at Columbia. At that time this institut offered anthropological work under several distinct auspic Ripley of *The Races of Europe* fame lecturing on that subj in the department of economics, while Livingston Farrand h forth on comparative sociology, religion, and art in t department of psychology. In 1889, however, Boas w appointed to head a new department of anthropology, wi Farrand as his adjunct. Two years later he also became Pt nam's successor at the American Museum.

His dual responsibility enabled Boas to bring studer into contact with anthropological collections and, above a to provide them with opportunities for field work under tl auspices of the Museum. During this period developed tl most ambitious research project of his career, the Morr K. Jesup Expedition, actually a series of expeditions design to shed light on Asiatic-American relationships. Boas's co laborators included Farrand, Harlan I. Smith, and oth Americans, as well as several noted European scholars, suc as Waldemar Bogoras, Waldemar Jochelson, and Berthol Laufer. In this connection and later Boas evinced a ra capacity for enlisting the coöperation of men qualified t advance science. It was during his curatorship, too, that Rolan B. Dixon, assisted by A. M. Tozzer, undertook the first strictl scientific investigation of a Californian tribe, culminating i the model monograph on *The Northern Maidu.* Even un academic men—intelligent whalers, such as Captains Mutcl and Comer—were drafted to make systematic observations o the Central Eskimo.

Several students, subsequently distinguished in the science won their ethnographer's spurs under Boas's jointly curatoria and professorial tutelage—A. L. Kroeber, Clark Wissler William Jones. Another fruitful institutional connection

resulted from Boas's appointment (1901) as Honorary Philologist of the Bureau of American Ethnology. It facilitated the accumulation and ultimate publication of vast bodies of linguistic material, as evidenced in the *Handbook of American Indian Languages* (1911, 1922).

A clash with Dr. Hermon C. Bumpus, then director of the American Museum, concerning methods of installation and the generic issue of departmental autonomy, led to Boas's resignation (1905) as curator and for many years severed the intimate bonds of the Museum department with that at Columbia. However, he soon found other outlets for his surplus energy. In 1908 he became editor of the *Journal of American Folk-Lore;* in 1910 he helped create the International School of American Archaeology and Ethnology in Mexico; in 1917 he founded the *International Journal of American Linguistics;* and for many years he edited the *Publications of the American Ethnological Society.* In 1908, moreover, the United States Immigration Commission authorized him to undertake a somatological study of European immigrants. The task once more involved the careful planning of a large-scale project with the aid of many assistants. Nor did personal field work cease: he directed excavations in Mexico and Porto Rico, went to the Kootenay and to the Keresan Indians, even revisited the Kwakiutl in his old age. Besides all this he regularly attended scientific congresses in America and Europe.

Boas's many-sided scientific activities found national and international recognition. He was elected to the National Academy of Sciences in April, 1900; was a member of the American Philosophical Society; president of the American Association for the Advancement of Science in 1931 and of the New York Academy of Sciences in 1910. Among his foreign honors may be mentioned the doctorate bestowed by Oxford University.

World War I and its aftermath brought to the fore some little suspected facets of Boas's personality. He had long

in the first World War, should flout the principles dear to him was an unbearable thought. Besides, being of Jewish extraction, he had relatives in Germany whose very existence was threatened by the *Umbruch*. He reacted once more in character, writing an open letter to President Hindenburg, denouncing the tenets of Nazism in the daily press or in popular magazines; dragging himself when already enfeebled by old age and an encroaching heart disease to the platform at public gatherings in order to inveigh against Hitlerian excesses.

His campaign against racism naturally brought him a wider following than the monographs on the Kwakiutl or even the academic treatment of race in *The Mind of Primitive Man* (1911, 1938). He became the spokesman not only of disinterested humanitarians, but also of Leftists and Communists. Communists are not universally popular, and even in quarters averse to Nazi philosophy the association with them sufficed to make Boas a suspect fellow-traveler. The facts seem to be as follows. Boas had a live social sense that automatically made him favor the underdog, so that he was unquestionably a liberal rather than a conservative in his general outlook. On the other hand, he loathed regimentation, whether by a college president, a party machine, or an unenlightened public opinion. When Lily Braun, the renegade daughter of a Prussian general, published her memoirs in 1908, Boas read them and was repelled by their picture of Social Democratic party tyranny. In possibly my very last conversation with him, a year or so before his death, he broached the subject of the Bolsheviks, summarizing his position in these words: "The Communists have done many very good things, they have also done many very bad things." Assuredly this was not the voice of blind partisanship. As for Marxist doctrines, he had all his life recoiled from closed systems, hence could not accept a philosophy of economic determinism or any other dogmatic scheme. On the other hand, no one was less likely than he to avoid contacts simply because they might arouse

general disapproval. "Fellow-traveler", "pink", and "red" were to him meaningless catchwords.

On December 21, 1942 Boas was lunching with Professor Paul Rivet (Paris) at the Columbia Faculty Club in the company of several colleagues. The guest of honor has graphically recorded the experience (*Renaissance,* 1:313f., 1943). Boas had just voiced his contempt for racism, when the fatal stroke occurred: "Sans un cri, sans une plainte, nous le vîmes se renverser en arrière; quelques râles, un grand cerveau avait cessé de penser."

* * * * *

Boas's services to anthropology were so great and manifold that occasionally enthusiastic disciples unfamiliar with history talked and wrote as though his predecessors and contemporaries were negligible. One obituary article declared: "He found anthropology a collection of wild guesses and a happy hunting ground for the romantic lover of primitive things; he left it a discipline in which theories could be tested and in which he had delimited possibilities from impossibilities." This is to parade Boas as a mythological culture-hero creating something out of nothing. The conception would have been intolerable to Boas, who fully esteemed what had been done by E. B. Tylor, Lewis H. Morgan, Eduard Hahn, Karl von den Steinen, and others. Indeed, he was especially appreciative of men who had achieved what he himself never attempted—an intimate, yet authentic, picture of aboriginal life. I have hardly ever heard him speak with such veritable enthusiasm as when lauding Bogoras's account of the Chukchi, Rasmussen's of the Eskimo, Turi's of the Lapps.

In the following paragraphs, then, I shall try to sketch Boas's achievement in perspective and without unfairness to others.

* * * * *

To begin with an obvious fact, he approached the study of man from every angle: as Rivet puts it, "son oeuvre embrasse le problème humain dans son entier . . . . Tout ce qui concerne l'homme sollicite sa curiosité . . . ." What is

more, from the start he saw the need of acquiring in each branch of the science the highest degree of technical equipment. The physical anthropologist must use the tools of biometrics; the linguist must become a phonetician and an analyst along the lines of Indo-European philology; the ethnographer must envisage the subtler as well as the more obvious phases of social life—folk-literature, music, the subjective attitudes of primitive man no less than artifacts or social structure. Nothing is more remarkable than the systematic way in which Boas, trained in quite different fields, acquired the techniques requisite for the highest type of work in the several subdivisions of anthropology. Even in archeology, which he treated with comparative neglect, his work has been declared to show "a perfect appreciation of the problems and the best archeological techniques."

Further, this many-sided virtuosity was justified by the solidity of his results. Everywhere he saw new problems and devised new methods of attack. Even his archeological contributions, Mason assures us, "all have been substantiated by later and more detailed work. They have formed the basis for all later research in this region." What is more, they preceded by several years the stratigraphic approach that rightly shed luster on Kidder's and Nelson's work in the Pueblo area. Again, in linguistics, Boas was, if not the first, yet the most persistent "to analyze exotic material without forcing it into the strait-jacket of the familiar" (Emeneau). As a physical anthropologist he deprecated sheer taxonomy; defined race on a profounder basis; demonstrated the (*nota bene*, limited) plasticity of the human organism; studied the phenomena of growth on a major scale; and was one of the earliest investigators to note segregation in hybrid human groups. His ethnological contributions were so varied that two must suffice for purposes of illustration. He was the first to inquire into the aboriginal artist's subjective attitude toward his tasks; and, paralleling the work of Homeric scholars, he correlated the social life depicted in a people's

folk-literature with their observed culture. In theory he may be described as an epistemologist rather than a metaphysician: he suspected traditional labels and catchwords, inquired into their empirical foundation, and often arrived at a new and illuminating re-classification of data.

Tastes differ in science, as in everything else. Hence Boas's achievement was bound to disappoint certain minds. Keenly aware of the gaps in our knowledge, he refused to fill them with plausible speculations resulting in a spuriously complete picture of the whole field. He proclaimed no all-embracing "laws" and, except for his views on race, voiced no simple message that might appeal to large masses. In point of form he lacked the polished diction of a Frazer or the sprightly humor of his friend, Karl von den Steinen. Nor did he complete a single large-scale portrait of a tribal culture, not even of his beloved Kwakiutl.

Similarly, his teaching was not designed for everyone's palate. The most effective trainer of anthropological investigators was not an ideal pedagogue. He was, indeed, uncanny in his capacity to harness a student's skills for the advancement of science, but he did not trouble to ferret out a learner's needs at a particular stage of progress. Novices were not pampered with milk for babes. Fearful lest they turn dilettanti, he imposed on virtually every newcomer in my day his course on statistical theory (usually audited by professors from other departments) and another on American Indian languages. His ethnographic lectures rarely, if ever, systematically surveyed the area announced, but discussed the problems that engaged his attention. Other men's views he often treated in a way likely to mislead the immature, for by concentrating on controversial issues he sometimes conveyed the impression of total condemnation when there was merely partial dissent. One might easily carry away the idea that he had a low opinion of Tylor or Ratzel, as was certainly not the case. His critique of environmentalism, for instance, was urged so forcibly that for years I failed to grasp how carefully he

took cognizance of geographical factors. As to the skepticism he instilled by precept and example, he himself was at times smitten with qualms, wondering whether he was inhibiting the free play of the imagination, which, contrary to appearances, he rated very high. One student summarized his total reaction after a seminar of Boas's as follows: "All books are bad; articles *may* be good", the suppressed implication being that even they seldom were.

Yet he valued high-class work even when done by men of utterly different personality. Of Bogoras and Rasmussen I have already spoken. He keenly appreciated Francis Galton, William James, William Morton Wheeler, Karl von den Steinen. Of his Columbia colleagues I think he rated E. B. Wilson highest. "*He* is a first-rate man", he once said to me. Thomas Hunt Morgan he accepted as "very good", but with qualifications. Among Washington scientists, Karl Grove Gilbert enjoyed his esteem. Contrary to opinions occasionally heard, his scientific judgment was little warped by personal animosity. There was not much love lost between him and certain Washingtonian colleagues; but he described one of his bitterest enemies to me as a man of great native ability and gave another full credit for founding a technical journal.

To revert to his teaching, my novitiate probably came at the worst possible period for establishing rapport, for it was the time of his feud with the director of the American Museum. Boas seemed perpetually busy and preoccupied. I actually dreaded meeting him on the way to classes in Schermerhorn Hall. Utter silence would follow a curt "Good morning" till I found the situation intolerable. "Have you read Kollmann's article on Pygmies in the last issue of *Globus*?" I once asked him on one of these embarrassing occasions. He answered, "No"; I offered a few remarks on the subject; then we again walked on in silence. Having a conference with him was something of an event for A. B. Lewis, A. A. Goldenweiser, Paul Radin, and myself; Speck and Sapir, with their philological background, enjoyed, I think, a rather easier

entrée. This also had held true in Kroeber's and William Jones's time, and as a visitor in later periods I was able to watch his free and easy relations with subsequent generations of his disciples. Accordingly, I cannot but ascribe his earlier reserve to the tribulations of the era.

Systematic information, as indicated, he did not vouchsafe in ethnological courses, *that* the student was supposed somehow to get for himself. Yet it was not an easy task at a time when the good books had grown antiquated, so that trustworthy knowledge was obtainable only by wading through tomes of unilluminating descriptive detail. However, Boas was singularly unexacting in regard to a student's factual information. Probably there is not nowadays a single undergraduate major in any of our large anthropological departments who does not control a wider range of data than I did when Boas deemed me fit for the doctorate. It was enough that I had worked in the field, gained a theoretical conception there, and thrashed out the issue in a formal paper. On the other hand, he came very near holding up A. B. Lewis, whose knowledge was incomparably superior to mine, but whose dissertation discussed nothing of theoretical significance. Berthold Laufer, who liked it, observed querulously to me, "Boas always wants a thesis to have a point!"

Why did we reverence so indifferent a pedagogue as a great teacher? For the same reason, no doubt, that in later years mature men and women—Elsie Clews Parsons, Pliny Earle Goddard, and George A. Dorsey, for example—hailed him as their leader. Yet Goddard had come to New York full of skepticism about Boas; and Dorsey had been at swords' points with him in the American Anthropological Association. The explanation is simple. Here was a scientist primarily interested in science—not in the *organization* of research, not in the personalities of colleagues, not in a display of his cleverness, but in the problems that sprang from his data, in the quest of the truth. He seemed to personify the very spirit of science, and with his high seriousness—unsurpassed by any investi-

gator I have known in any sphere—he communicated something of that spirit to others. Therein lies his greatness as a teacher.

Constituted as he was, he could not avoid misunderstandings either as to his views or his character. Even scientific guilds live by slogans and balk at finer distinctions. Boas threw out a hint how totemism might have evolved in British Columbia and was forthwith credited with a universal theory of the phenomenon. Pointing to the positive achievements of colored races, he rejected the arguments of racists, hence was either hailed or denounced as a dogmatic equalitarian. Yet he clearly formulated in both editions of his most popular book a rather different position: "It may be well to state here once more with some emphasis that it would be erroneous to claim as proved that there are no differences in the mental make-up of the Negro race taken as a whole and of any other race taken as a whole, and that their activities should run in exactly the same lines" (*The Mind of Primitive Man*, New York, 1938, p. 270). Again, his championship of a strictly limited plasticity was misinterpreted as a denial of heredity. Some forty years ago, at a joint meeting of anthropologists and psychologists, even his friend, James McKeen Cattell, contrasted Boas's environmentalism and Thorndike's emphasis on heredity. Boas was at once on his feet, protesting that he, too, attached very great importance to heredity.

Boas's aversion to systems and sweeping generalizations lent color to the charge that he was absorbed in detail—content, like Browning's Grammarian, with settling *Hoti's* business and giving the doctrine of the enclitic *De*. For, sharing Bastian's and Haddon's eagerness to rescue rapidly vanishing data, he did devote enormous energy to securing and making accessible raw material. It is easy to go through a thousand pages of his monographs without encountering a line of interpretation. But that was only one side of him and, of course, the least interesting. The faithful recorder was, above all, a thinker. I remember his suddenly electrifying a seminar with

the statement that he was concerned with detail only as a way to understanding human mentality. On another occasion he quoted von den Steinen's saying that we must look at primitive man without the spectacles of our civilization; Boas amended it to read that we must look at *ourselves* without spectacles. He was ever aware of the preconceptions with which, as Virchow once put it, we are all "crammed full from infancy on." He once told me how hard he had had to struggle to overcome early rationalistic influences; and the burden of all his ethnological teaching, paralleling his linguistic position, was that every philosophy and form of behavior must be apprehended from the insider's point of view.

As explained, he was not a doctrinaire on the subject of race. It is worth adding that he was not a sentimentalist either. He befriended Indians, but unless (like Jones) they had qualified academically he did not welcome them to his classes. Similarly, he was strongly suspicious of any prospective disciples who were goaded by a romantic interest in the noble Red Man rather than by the urge to advance knowledge.

Notwithstanding his neglect of customary canons of presentation, Boas was far from lacking in aesthetic appreciation. Characteristically his abiding ethnological interests were primitive art and oral literature. A devotee of music, he played the piano very well for an amateur. Above all other composers he revered Beethoven. "To think that it was possible for such greatness to exist!" I have heard him say. Chopin, on the other hand, repelled him as morbid. In literature he naturally admired Goethe. Some years ago I asked him whether he still occasionally read him. "Of course," was the instant reply. Sheer wit or glamor had no appeal. George Bernard Shaw palled on him. I cited Heine as a parallel, only to have my defense parried with "Well, doesn't *he* tire you?" He was also very critical of Max Reinhart's staging, but somewhat grudgingly admitted the effectiveness of his production of Büchner's *Danton.*

That Boas made enemies is an empirical fact. Its expla-

nation lies partly in circumstances, partly in his personality. For any adult immigrant to adapt himself fully to the folkways of his adoptive country requires a prodigy of flexibility and tact, especially in the peculiarly exacting atmosphere of an Anglo-Saxon society. Boas often appeared ruthless when from his own point of view he was merely candid. He could certainly be very blunt when matters of principle were involved, but then the personalities who make history have rarely been marked by a dainty concern for the sensibilities of queasy souls. On the other hand, no one could be more understanding and kind in basically human situations. As the factotum of the department at the American Museum once confided to me, Dr. Boas had been a strict master, but a generous contributor to any employees' fund. Once, too, Boas disposed of some of his insurance in order to aid a former student in an alleged crisis; he entered with fullest sympathy into the feelings of a young lover or newlywed; and no one could write tenderer notes of condolence on the occasion of a bereavement.

But where no fundamental human factor was involved, empathy was too readily blotted out by contrary emotional urges. In a seminar he once referred to a map in the Swedish journal *Ymer*. "What language is the article in?" asked a student of great erudition, but little initiative. "The map can be understood independently of the language," Boas snarled back. Estrangements from one-time students were in part merely the familiar phenomenon of filial revolt, but in part they resulted from Boas's taking a rational point of view that clashed with the disciple's emotional urges. He was wont to survey the chessboard of anthropological jobs and figure out how science could be best served, then he would try to move anthropologists about like the pawns in a game. His judgment was usually right, but some men and women resented the impersonality of his strategy. One case may be cited as typical. He had secured for an outstanding student an excellent position with superb research opportunities, but in a city without a university and meager in cultural facilities. After a few years

the incumbent grew restive, felt marooned, and eagerly accepted a metropolitan appointment—much to Boas's surprise and disgust. The master found it difficult to understand that extra-scientific motives should have tipped the scales.

Deficiency in empathy was naturally intensified when principles seemed at stake. It was not easy for him to do justice to an ethically uncongenial attitude. In 1919 he excoriated four anthropologists who had mingled intelligence work with research in Latin America during the War. It did not occur to him that, from their point of view, they had been merely discharging a patriotic duty. In other cases he was unwilling to make allowances for human frailty. In 1933 he could not understand the conduct of Germans who welcomed Hitler even when they repudiated his racist programme. The point is not that he disapproved, but that he seemed unable to project himself into the mental state of men who were at once kindly and fervidly patriotic, who were in other words caught in a fearful conflict of humanitarian and nationalistic loyalties. He forgot that one cannot expect every man to be a hero.

Such absorption in his own ideals was, of course, from another angle part and parcel of his greatness. In trying to boil down my admiration for him into a few words, I find that I have been forestalled by Wundt's eulogy on an otherwise very different personality, Gustav Theodor Fechner: "absolute lack of bias [due to tradition] and intrepidity." The mere fact that a view was universally accepted and supported by eminent authority was precisely a ground for skepticism in both Fechner's and Boas's case. Observations Boas had not himself made he was likely to challenge or at least to mistrust. Meeting him in Berkeley in 1914, I dropped the innocent remark that there were many tall women about town. Without directly denying the statement, Boas pooh-poohed its significance: "In a population whose males average 175 cm [he was never at ease with feet and inches] you must expect to find some tall women." A few days later he remarked, "Aren't

there many tall women in Berkeley?" He had by that time observed .for himself.

Qualities and men are rightly prized for their rarity. Boas had unusual intellectual powers and extraordinary devotion to science, yet if I were to single out his unequivocal claim to greatness I should rather stress the qualities he shared with Fechner, for it was these that mark him as a figure to be aligned with those who have made human history. The correctness of his attitudes seems quite immaterial; what counts is his remaining true to his vision, with total disregard of whether the mob stigmatized him as "pro-German" at one stage or as "Communist" at another. To have known such a man in the flesh is what I esteem above any of his specific teachings, as, once more to quote Wundt, "an inalienable gain of my life" (*einen unverlierbaren Gewinn meines Lebens*).

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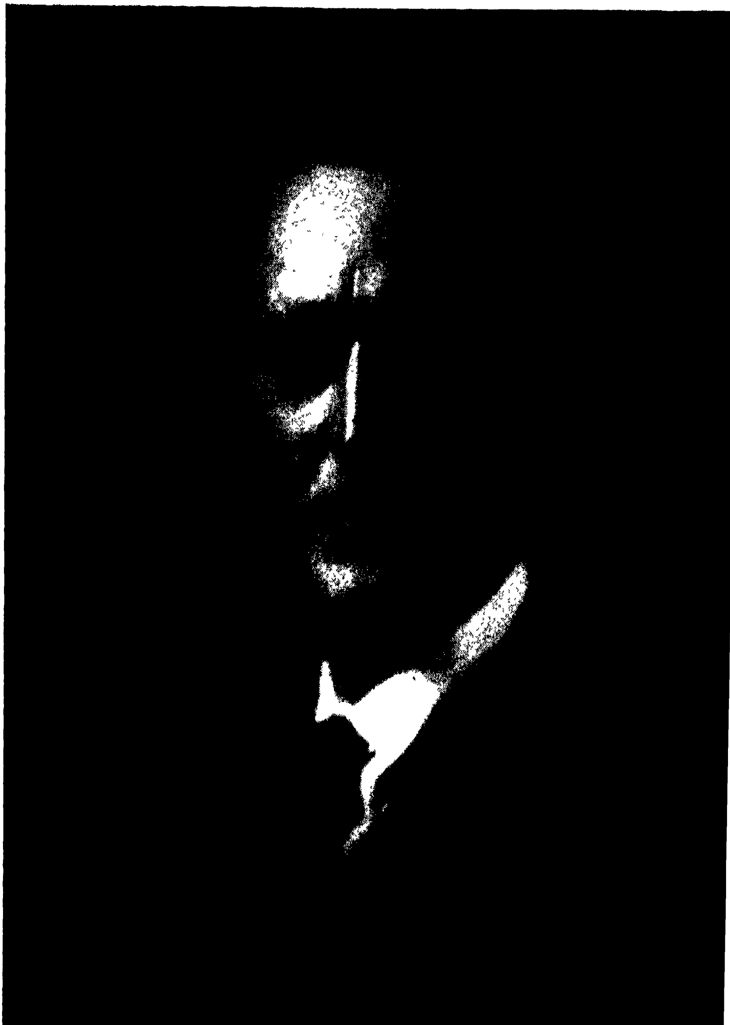

NATIONAL ACADEMY OF SCIENCES

OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XXIV—TENTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

HANS ZINSSER

1878–1940

BY

SIMEON BURT WOLBACH

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1947

HANS ZINSSER

1878–1940

BY SIMEON BURT WOLBACH

Hans Zinsser was a compound of many talents, productive in science, education, literature, and poetry, and apparent in music, art, and sports. He had, moreover, an unusual capacity for making close and lasting friendships, a capacity arising no doubt from his own warm quality of friendliness and his generosity, tolerance, compassion, sense of fair play and courage. In any walk of life his alert, agile and able mind and splendid physique would have distinguished him. He was exceptionally fortunate in early educational advantages and home life, all quite different from those of most American-born scientists.

Outline of Dr. Zinsser's Life

Dr. Zinsser was born in New York City on November 17, 1878, into a German family of affluence and culture. On both sides his forebears were freedom-loving, independently thinking people. His father, August Zinsser, a manufacturing chemist, came from the Rhineland; his mother, Marie Theresa (Schmidt) from the Black Forest region which "had long been under the influence of French thought and political doctrine."

Dr. Zinsser was eight years younger than the next of three older brothers and because of this gap the attention and care he received was much like that given to an only child. The early environment provided him by highly educated and cultured parents who were devoted to each other was ideal. A country home in Westchester County, New York, gave out-of-doors pursuits and contact with domestic animals. Horses for driving and riding were at hand and he very soon acquired proficiency in horsemanship and a love of horses which lasted throughout his life. In later years he was a skilful and fearless rider to hounds. His early education, which included study of the violin and piano, was received at home from tutors and an uncle who was a physician and a musician as well. While an infant he

made his first trip abroad and travel in Europe was a yearly event until he was twenty years of age. On many of these excursions he was under the guidance of his cultured uncle and thus was thoroughly and tutorially exposed to the best in European art galleries and concert halls. Two early years were spent in school at Wiesbaden where this uncle lived. In Germany, France and Italy he also lived and travelled with his parents. At home, up to the age of ten, only German was spoken. At this age his first formal schooling began. The school, a private one in New York City, run by Julius Sachs, was highly esteemed by educated Germans. His training here was almost wholly in the liberal arts. At the age of seventeen (1895) he entered Columbia, imbued with a desire for the study of literature and for a career as a writer. At that time the elementary courses were given by the heads of the departments, so that he came into close contact with Professor George Edward Woodberry of the Department of Comparative Literature, from whom he acquired, through inspiration and criticism, invaluable facility and standards in composition of poetry and prose. In his junior year he took courses in biology under Edmund B. Wilson and Bashford Dean.

Up to this time the world of Hans Zinsser had been pretty much a world of things and thoughts created by the minds of men. He had been trained in the appraisal and appreciation of literary and artistic standards and opinions, sifted and accepted by the best intellects. Now, suddenly, a new world was exposed to him by Wilson and Dean whom he regarded as the ablest teachers he ever had. While not losing his literary aspirations, the biological sciences became his greatest interest. Ideas and convictions absorbed in earlier conversations with his agnostic father may well have fixed in him a naturalistic attitude which may in part account for this deviation of interests. Later he turned from general biology to medicine, a natural transition because Hans Zinsser was a practical-minded person who throughout his life aspired to useful results from his research.

The Spanish War led to his enlistment, during his third

year in college, in a cavalry unit (Squadron A) composed mostly of college men. After two years in this unit he got himself transferred to what he thought "was the toughest outfit in New York" and achieved thorough and revealing contacts with such American youth—offspring of many nationalities—as were mainly the product of city environment. He acknowledged the value of this experience in preparation for later adventures. At the end of his third year in college, three months spent on a paleontological expedition for Professor Osborn in the Southwest added to his scientific knowledge, gave him time for a thorough study of the Bible, and perhaps of greatest importance, added to his self-reliance because of the hardships and the variety of acquaintances incidental to a trip of this sort. It is a good guess that by this time his very carefully nurtured early education was well counterbalanced, and that his interest in people had spread to all levels.

During a college vacation when he was twenty, he took a walking trip through France and, according to Mrs. Zinsser, "his ardent admiration for things French and for France herself date from that time." He took no part in college athletics. He learned to fence in France when very young and fencing continued to be, with riding, one of his favorite exercises throughout his life.

Thus, when he began the study of medicine at the College of Physicians and Surgeons of Columbia University in the fall of 1899, he was proficient in German and French languages, thoroughly self-reliant and enthusiastic, probably more European than American in his point of view, yet possessed beyond most youths of attributes regarded as American—independence of thought, competency with horses and firearms, and in dealing with persons of all social levels.

While in the Medical School he completed his thesis for the M.A. degree on the early embryology of the mouse, and did extra work in bacteriology. His first scientific publication appeared in 1903, the year he received the degrees of M.A. and M.D. The years 1903 to 1905 were spent interning at the Roosevelt Hospital, after which he made a half-hearted

venture in the practice of medicine while remaining connected with the Roosevelt Hospital as Bacteriologist, and with Columbia University as Assistant in Bacteriology. In June 1905 he married Ruby Handforth Kunz of New York.

When in 1908 he received a full-time appointment at Columbia as Instructor in Bacteriology, he abandoned the practice of medicine with alacrity. This period in bacteriology at Columbia was a busy one. From 1907 to 1910 he was Assistant Pathologist at St. Luke's Hospital, New York. He was active in research, alone and in collaboration with Dr. Philip Hanson Hiss. With Dr. Hiss he also published the celebrated "Textbook of Bacteriology" now in its eighth edition. Most of the actual writing of this book was delegated to Dr. Zinsser because of his relish and facility for the task.

In 1910 he went to Leland Stanford University as Associate Professor of Bacteriology and Immunology. In 1911 he was appointed to the full professorship, which he held until 1913 when he was recalled to Columbia University as Professor of Bacteriology and Immunology.

A. W. Meyer, Professor of Anatomy Emeritus, has written a brief and entertaining account of "Zinsser at Stanford." From this source and from Mrs. Zinsser we learn that the years 1910-1913 at Palo Alto were very happy ones and, of course, very busy ones. After his arrival a laboratory was rapidly equipped for him in a building belonging to the Department of Anatomy. Some of his equipment was improvised and Mrs. Zinsser relates that she and Dr. Zinsser made many expeditions at night by horse and buggy when he was disturbed about the reliability of the bacteriological incubator. Dr. Zinsser himself helped build an animal house and an enclosure for goats, sheep and horses, animals necessary for his immunological research. Classes were small but composed of able students who gave gratifying response to Dr. Zinsser's enthusiastic and stimulating lectures. Professor Meyer describes him in 1910 as ". . . very youthful in appearance and the father of an engaging daughter of about two. He was slight of stature with light blond hair and scarcely evident eyelashes and eyebrows, frank

and exceptionally energetic and alert. His reactions not only were immediate, but sometimes surprising. Though full of life and mercurial in temperament, when serious he looks somewhat troubled. It soon became evident that President Jordan had surmised correctly that he was a 'live wife' and that he 'probably would go farther' than some others considered for the position." At Stanford Dr. Zinsser made many friends. He left them and his horses with regret when he took his family, his foils, and his violin back to New York. In his autobiography "As I Remember Him" he refers affectionately to "happy Stanford" and to his beloved friend, President Jordan.

Now followed ten productive years from 1913 to 1923 as Professor of Bacteriology and Immunology at Columbia University. During this decade he took part in the first World War as a member of the Red Cross Typhus Commission to Serbia in 1915 and in 1917-1919 as an officer in the Medical Corps of the United States Army. The Serbian expedition was scientifically unsuccessful but it brought Dr. Zinsser into close contact with "mass misery" and he characterized the typhus epidemic there as "as terrifying and tragic an episode as has occurred since the Middle Ages." Eight years later, in 1923, in Russia on a Sanitary Commission for the League of Red Cross Societies, he again encountered epidemic typhus. From these experiences he received the inspiration and material for his book "Rats, Lice and History" published in 1935, and out of them an objective which led him to concentrate on immunological research on typhus during the last ten years of his life.

His war record was distinguished. He was commissioned Major in 1917 and Colonel in 1918. He was with the A.E.F. in France for two years as Sanitary Inspector of the First Corps, and later of the Second Field Army. For a period he was Assistant Director of the Division of Laboratories and Infectious Diseases. Bayne-Jones has written: "The thoroughness of his inspection under conditions of danger and discomfort and his lashing condemnation of breaches of sanitary regulations are vividly remembered by all who saw him in action. With his usual comprehension he grasped the breadth of the problems

of military sanitation, understanding their relationship to the hygiene of the individual and to general public health. The orders issued on his recommendation in the Second Army became a treatise, published in 1919, entitled 'The Sanitation of a Field Army.' " After the war he was awarded the Distinguished Service Medal. In the accompanying citation are these statements: "For exceptionally meritorious and distinguished services. While acting as Sanitation Inspector of the Second Army he organized, perfected and administered with extraordinary and exceptional success a plan of military sanitation and epidemic-disease control." Interest in military sanitation persisted and his last publication, which appeared in 1940 just before his death, was "On the Medical Control of Mobilization." In this paper he advocated procedures which he believed would prevent the occurrence of disastrous epidemics.

At Columbia, as always, his research was chiefly in the field of immunology. The years 1914-1916 were largely given to the study of the *Treponema pallidum* by cultural methods and to problems of immunity to syphilis in animals.

The first edition of his textbook on immunity, now in its fifth edition, was published in 1914 with the title "Infection and Resistance." "A Laboratory Course in Serum Study" was published in 1916.

In 1923, at the age of 45, he came to Harvard University Medical School as Professor of Bacteriology and Immunology, and in 1925, on the retirement of Dr. Rosenau, received an added distinction at Harvard by having the Charles Wilder Professorship transferred to him.

At Harvard Dr. Zinsser flourished to his full capabilities. He became a University figure because his diverse interests and many talents brought him into contact with the outstanding intellects in many departments and all branches of biology, philosophy, social science, and education. He was a rapid and omnivorous reader. He acquired a reputation for his judgment of literature and, in Cambridge discussions, for his stimulating comments "in a bewildering variety of subjects." His first appearance as a speaker on non-technical subjects was his delivery

of the "Ether Day" address at the Massachusetts General Hospital—always a distinguished occasion—in 1924. In after years he was in considerable demand as a speaker for academic occasions and he gave numerous notable addresses on education, general and medical.

His department always presented a busy appearance. Pupils of many nationalities came to work with him. Research in immunological fields remained Zinsser's major interest until 1930, after which typhus fever absorbed completely his scientific endeavors. Superfluous intellectual energy went into writing of prose and poetry. He contributed articles frequently to the Atlantic Monthly, his first in 1927 entitled "The Perils of Magnanimity, a Problem in American Education" was admonitory in tone regarding "The Foundations" and in particular the Rockefeller Foundation. This short article is illustrative of Zinsser's self-reliance in judgment and courage in expression of convictions that characterized his academic activities at Harvard. Perhaps it illustrates his wisdom for, in the opinion of many, some of the perils he indicated have materialized.

Dr. Zinsser's bibliography indicates how he ripened at Harvard in literature and in science. His addresses on education reflect his own wide experience in teaching and give evidence of his erudition. Much that he wrote is epitomized and in some instances expanded in his autobiography "As I Remember Him." His continuous research on immunological problems was well qualified to make him appreciative of the complicated interplay of factors, those amenable to experimentation and those dependent upon human qualities, good and bad. He was as critical of premises obtained from observation of human affairs as from his laboratory work. His writings indicate that he was as much concerned over the evil operations of men's minds as was Mark Twain but he was cheerful about it and on the whole optimistic, at least never gloomy. In his books, "Rats, Lice and History" and his autobiography, his hopes and thoughts about humanity were revealed. What some of them were can be indicated best by quoting from Charles Sedgewick Minot (The Problem of Age, Growth and Death. Lowell Institute Lectures, delivered

March 1907). "The time will come, I hope, when it will be generally understood that the investigators and thinkers of the world are those upon whom the world chiefly depends. I should like, indeed, to live to a time when it will be universally recognized that the military man and the government makers are types which have survived from a previous condition of civilization, not ours; and when they will no longer be looked upon as the heroes of mankind. In that future time those persons who have really created our civilization will receive the recognition which is their due. Let these thoughts dwell long in your meditation, because it is a serious problem in all our civilization today, how to secure due recognition of the values of thought and how to encourage it."

After Zinsser concentrated on typhus fever research, he made many visits to Charles Nicolle in Algiers. A friendship begun in 1928 in Tunis ripened into mutually great affectionate regard. These two men, both versatile in literature and science, had everything except years in common. Of Nicolle's death, Zinsser wrote: "It was the same order of sorrow as had been the death of my father."

Up to his death, Zinsser was active in the laboratory. His final goal was an effective vaccine against typhus fever. In 1935 he was sent by Harvard University to France as Exchange Professor. In 1938 he was an Exchange Professor at Peiping University Medical College.

Abounding in energy and seemingly physically and mentally tireless, life at Harvard was a full one. In addition to a house on Chestnut Street, Boston, the Zinssers had a farm at nearby Dover, where entertainment of a vigorous sort was provided for members of his department and for medical students. Until his last illness he rode to hounds with the Groton Hunt, and in spite of long working hours in the laboratory he kept lean, muscular and tanned. When perplexed or satiated with work he fiddled. Apparently as a minor side-issue he kept his textbooks up to date. There were always in his room, upon a stand-up desk built for his predecessor Dr. H. C. Ernst, high piles of great sheets of paper, large enough to provide for foot-wide margins around the

printed pages of text which he pasted upon them as soon as a new edition was printed. The wide margins provided ample room for recording instantly the changes and additions suggested by publications as they appeared.

Dr. Zinsser's Scientific Work

Dr. Zinsser's two textbooks must be included as part of his scientific work. His "Textbook of Bacteriology," first published in 1910 with Philip Hanson Hiss, Jr. and finally with Stanhope Bayne-Jones, has gone through eight editions and thirty-eight printings, and has been translated into several languages, including Chinese.

"Infection and Resistance" which was published in 1914, went through three editions; a fourth edition in 1931 was published under the title "Resistance to Infectious Diseases" and in 1939 the fifth edition, written in collaboration with John F. Enders and LeRoy D. Fothergill, appeared with the title "Immunity: Principles and Application in Medicine and Public Health."

Both these books and particularly the one on immunology are examples of what textbooks should be because they present in an integrated and clear manner that which is known or believed and stimulate interest and curiosity by revealing the problems still unsolved.

Although Dr. Zinsser made contributions of etiological significance for syphilis, rheumatic fever, and typhus fever, he worked consistently throughout his career in the field of immunology. In a list (perhaps incomplete) of 106 papers recording actual research, 70 deal with subjects in immunology; over half of these (43) are concerned with fundamental aspects of immunity; the remainder with immunological problems of pyogenic diseases, tuberculosis, syphilis, typhus and virus diseases.

Dr. Zinsser's first paper, in 1903, was upon the effects of radium on bacteria. During the next five years his publications dealt with laboratory methods and interesting examples of infection in man. From 1908 to 1911 he collaborated in research with Dr. Hiss on the preparation of extracts of leucocytes and on the application of these extracts of leucocytes to the treat-

ment of infections in man. They showed that the extracts did not promote phagocytosis and that their moderate bactericidal activity could not account for their effectiveness. In reviewing this work, Dr. Zinsser wrote in 1940 that he was "inclined to believe that the beneficial effects are to be attributed to those obscure factors which account for the not infrequent successes of so-called nonspecific protein therapy, which consists in the injection of almost any bacterial or other protein."

Zinsser's studies of fundamental problems in immune phenomena began at about the time he went to Leland Stanford University in 1910 and studies of the precipitin reaction made him realize that antigen-antibody reactions should be amenable to elucidation by physical-chemical methods particularly as applied to colloïdal chemistry. This led to a close association with Stewart Young, the professor of Physical Chemistry and the publication of important papers by them on the precipitin reaction. At this period Zinsser " . . . often expressed a very strong belief that physical chemistry would ultimately explain a great many things in the field of immunology" and, aware of his inadequate knowledge of mathematics, he plunged into the study of algebra and trigonometry in preparation for the mastery of calculus because, as he said, "If I am ever able to do anything with bacteriology I have got to be able to handle my physical chemistry like a man, not like a . . . fool." Unfortunately, Zinsser did not devote enough time to become greatly competent in physical chemistry, which may be explained in part by his return to Columbia and new interests in problems presenting practical objectives. As a result of this excursion into physical chemistry he became one of the earliest proponents of the "unitarian view" concerning the essential identity of the antibodies, a subject which he has succinctly dealt with in historical perspective in his textbook on immunity. In 1919, his interest in the physical-chemical aspects of immunity also led to the addition of a biochemist to his department at Columbia. The appointee, J. Howard Mueller, remained with Dr. Zinsser until the latter's death, and succeeded to the Professorship of Bacteriology and Immunology at Harvard. Years later Zinsser was able to apply successfully his

earlier acquired physical-chemical knowledge to the determination of the size of virus particles by filtration through membranes of graded porosities and to methods for determining the metabolic activity of cells in tissue cultures most favorable for the cultivation of viruses and rickettsiae.

An objective of Dr. Zinsser on returning to Columbia was the achievement of a method of active immunization against syphilis, apparently a possible goal because of "Noguchi's success in cultivating spirochetes and his observations on reactions produced by the injection of luetin. . . ." "A series of papers was published during the years 1914 to 1916 on the cultivation of Spirochaeta *(Treponema pallidum)* pallida, on fluctuations on the virulence of these organisms, on spirochetocidal antibodies and on phenomena of immunity to syphilis in animals." He did not succeed in his objective, a method for immunization against syphilis, but he added to the knowledge of spirochetes and disclosed problems presented by the differences between the properties of spirochetes in cultures and virulent ones from human lesions that are still unresolved, including the question that prevails to this day—actual proof of the cultivation of *Treponema pallidum.*

Dr. Zinsser, in 1914, was one of the first investigators to study the effects of heat upon the behavior of proteins used to immunize animals, and to show that a heat-resistant fraction was concerned with the specific nature of the resulting immune body. His later tuberculin studies led to a generalization of first importance, applicable to responses in bacterial infection in general. This generalization can be expressed by quoting from his 1921 paper. "It would appear that certain non-coaguable substances of uncertain chemical constitution are being constantly elaborated in the course of bacterial growth and passed into the circulation of infected animals. As a result of this, infected animals become sensitized to the heat and acid-resistant materials. Early in the course of infection the animal becomes sensitized and subsequently the further elaboration and distribution of these materials from the bacterial focus play a fundamental part in the injury of the animal. These proteose-like substances, like tuber-

culin, possessing but slight toxicity for the normal animal, become highly toxic to the sensitized one. Thus these substances, while not being true exotoxins in the ordinary sense, would still represent a highly toxic bacterial product comparable in its injurious effect to toxins when produced in the body of an animal thus sensitized. . . ." "If there is any value in these deductions, the attention of bacteriologists should be turned to the non-protein constituents of bacterial cells in their further immunological studies, as well as to the protein materials."

This generalization of Hans Zinsser's is perhaps his greatest in the field of immunology. It reflects his knowledge of pathology and his broad understanding of the sequences that compose a disease entity. "Residue antigens" was the name he gave to these "heat and acid-resistant materials." He and his associates and many others confirmed and extended his observations, now of very great theoretical and practical importance in the elucidation and serum treatment of bacterial diseases. Through the work of Heidelberger and Avery we know now that at least certain of Zinsser's "residue antigens" are polysaccharides.

During the period 1926 to 1930 he published a number of papers on the virus of herpes and on immunologic studies of the herpesencephalitis problem. The results, together with those of others, are summarized in a paper entitled "An Immunological Consideration of the Virus Problem" published in 1936. While working with the virus of herpes he became interested in the sizes of virus particles. "Finding no precise information on this subject in the literature, he undertook with Tang to make the necessary determinations by measuring viruses against the permeability of graded filters made of collodion by a method more or less analagous to that which Bechold used in an attempt to establish a scale of sizes for various substances ranging from crystalloids to Prussian blue." "The results were published in 1927. He estimated that herpes virus, Rous sarcoma virus and bacteriophage 'were of a magnitude larger than casein and collargol and smaller than colloidal arsenic.' This range of sizes was from 20 to 100 millimicrons. Considering the difficulties and uncertainties, these determinations are amazingly close to

the value accepted at present, namely: for bacteriophage, 75 millimicrons, for the Rous sarcoma virus, 70 millimicrons, for herpes virus approximately 130 millimicrons. In this original work Dr. Zinsser was the first to determine the approximate size of virus particles by filtration through membranes of graded porosities." [1]

It is not advisable to attempt to outline systematically Zinsser's numerous papers pertaining to anaphylaxis and allergy. He was, however, the first to formulate a clear distinction between the tuberculin type of allergic response and classical anaphylactic shock as seen in animals. He pointed out that the tuberculin reaction—also allergic in nature—could be elicited only after a primary tissue reaction induced by the presence of the tubercle bacillus itself, in the animal. With the late Dr. Francis Grinnell he showed how a similar type of reaction could be produced with the unaltered proteins of other bacteria, such as the pneumococcus. He was convinced that allergic reactions were responsible for the lesions of some diseases and specifically, as a result of sensitization by the streptococcus, for those of rheumatic fever. Although not yet proved, this conception of the etiology of rheumatic fever still appears to many investigators as the most probable mechanism whereby the characteristic lesions of this disease are established.

Section I of his textbook with Enders and Fothergill is probably the best exposition of the "Principles and Theory" of immunity that can be found in a single volume in English. The five chapters devoted to "hypersensitiveness" illustrate Dr. Zinsser's absorption in this field and contain his own contributions fairly and appropriately presented and appraised in relation to the present day knowledge.

Dr. Zinsser's name and scientific reputation today are associated perhaps too largely with typhus fever. Substantial and serviceable as were the original contributions in this field by him and his associates, the writer appraises more highly the earlier products of his research. His fascinating books, "Rats,

[1] Biographical sketch by Professor S. Bayne-Jones in the Archives of Pathology, Vol. 31, p. 269, 1941.

Lice and History" published in 1935, and his autobiography, "As I Remember Him" published in 1940 compel emphasis upon lay and medical readers of his typhus work. Of his Serbian experiences as a member of the American Red Cross Sanitary Commission, organized by Richard P. Strong, he wrote in his autobiography: "My work at this hospital led to little immediate discovery. I gathered a great deal of information about the clinical aspects of the disease, did a great many autopsies, and learned the things that one can learn about typhus by living in an epidemic region. But scientific studies were hampered, not by any lack of opportunity or equipment, but rather by the fact that in typhus investigations at that time there was much underbrush to be cleared away. Before the true causes of the disease were uncovered, almost every known microorganism had, at some time or other, been implicated."

Apparently in Serbia in 1915, Zinsser elected to follow wrong trails. Nicolle, Comte and Conseil in 1909 had established that epidemic typhus was louse-borne and in 1910 Ricketts and Wilder proved that Mexican typhus or tabardillo could be transmitted from man to man in the same manner. They also found, in the bodies of infected lice, microorganisms which they regarded as the cause of typhus and which by their description were in all probability *Rickettsia prowazeki*. In 1911, Nicolle, Conseil and Conor showed that guinea pigs were susceptible to typhus.

In 1930, when Zinsser returned to typhus fever, *Rickettsia prowazeki* was the proved cause of typhus and had been grown in tissue cultures. Typhus unassociated with lice had been revealed in the United States and early in 1931 the virus was found in rats trapped in Baltimore and later in the year the rat flea was proved to be the carrier. Differences in animal reactions to European or epidemic typhus on the one hand and to murine typhus and tabardillo on the other had been described by Mooser. Mexican typhus and murine typhus produced in guinea pigs a much more severe inflammatory reaction in the tunica vaginalis after intraperitoneal inoculation than did European typhus. Also in contrast to European typhus, Mexican and murine typhus

infected guinea pigs showed great numbers of rickettsiae in the inflammatory exudate. Indeed, by 1930 there was an extensive literature dealing with the two diseases in which rickettsiae had been shown to be the pathogenic agents, Rocky Mountain spotted fever and typhus, and with rickettsiae in general, pathogenic and non-pathogenic alike. An effective vaccine containing rickettsiae obtained from the ground viscera of infected ticks and killed by phenol, for immunization against Rocky Mountain spotted fever, was in use.

There existed the problem of identity or non-identity of Old World or louse-borne epidemic typhus and the New World non-louse-borne endemic typhus, and the relation of epidemic louse-borne typhus in Mexico to both. Also, there was the problem of Brill's disease, sporadic typhus, apparently not louse-borne, which existed in cities in Northeastern United States, but restricted to emigrants from typhus regions of Europe.

The great objective of Dr. Zinsser and his numerous associates was the production of an effective vaccine for the prevention of typhus. In 1931, shortly after Dyer and others of the United States Public Health Service found typhus virus in Baltimore rats he, with Castaneda, proved the presence of the virus of Mexican Typhus in rats in Mexico City. Also in 1931, with Mooser and Castaneda, he showed that the rat louse, *Polyplax spinulosus,* could, as well as the rat flea, transmit the infection in rats. By reasoning based on epidemiologic data and by direct isolation of rickettsiae from patients with the disease, he showed that Brill's disease was typhus of the European type and came to the conclusion that it represented a recrudescence of typhus infection acquired in Europe prior to emigration.[2] His studies indicate, therefore, that man may very well be an epidemic reservoir of typhus for a period of ten to thirty years following infection. The implications of this conclusion are of very great importance.

Zinsser and Castaneda began studies in 1930 directed toward

[2]Recent investigations by Plotz employing the differential technique of complement fixation to distinguish between epidemic and murine typhus have confirmed Dr. Zinsser's important and pioneering observations.

the production of a vaccine against typhus. For this purpose great numbers of rickettsiae would have to be made readily available. The only vaccine then in use (since 1920) was that of Weigl, prepared from lice artificially infected by injection of rickettsia into the gut by means of a fine pipette introduced through the anal aperture. These lice then had to be nurtured for a week or more upon typhus-immune persons. Since it required from 50 to 100 lice to produce vaccine sufficient for the immunization of one person, the method obviously was not practicable for large scale use.

By subjecting rats to injurious procedures in order to reduce their "resistance" to typhus, Zinsser and Castaneda succeeded in 1933-34 in obtaining huge numbers of rickettsiae in the peritoneal exudates following inoculation with murine typhus but not with European typhus. Vitamin deficient diets, benzene poisoning and exposure to x-rays were used to decrease "resistance"; the last method proved most efficacious. The rickettsiae thus obtained, killed with formaldehyde, met the experimental tests for an efficient vaccine. This type of vaccine was used in human beings by Castaneda in Mexico and by Veintemillas in Bolivia. Zinsser and Castaneda also used formaldehyde-killed rickettsia obtained in the same manner for the production in a horse of an immune serum which was later used in Mexico "with a fair degree of success in preventive and therapeutic trials." Because this serum, though giving protection to guinea pigs inoculated with murine typhus gave only partial protection against the European strain, no beneficial effect was expected in patients with European typhus.[3]

Because the method of x-ray radiation failed to provide suspensions of European rickettsiae sufficient for the preparation of vaccine, tissue culture methods for the growth of rickettsiae were the next recourse of Dr. Zinsser. With Fitzpatrick and

[3] Recent results of the treatment of rickettsial diseases, Rocky Mountain spotted fever and typhus, with "hyperimmune" sera obtained from rabbits produced by repeated injections of rickettsiae, indicate that such sera are of value only in very early stages of the disease and that only in exceptional instances is diagnosis established sufficiently early for their use.

Wei, a medium was devised (1937-38) which was superior to those hitherto employed. This medium was essentially a modification of the Nigg-Landsteiner modification of Maitland's medium; the essentials of which were Tyrode's solution, blood serum and appropriate minced tissue devised for the cultivation of vaccinia virus. Nigg and Landsteiner had found that *Rickettsia prowazeki* could be cultivated for many generations *in vitro* "in media similar to those described by Maitland, Rivers and others for the cultivation of certain viruses. In all probability such cultures can be maintained indefinitely." The Zinsser-Fitzpatrick-Wei medium is essentially the Maitland medium sterilized by filtration and solidified by the addition of isotonic agar jelly. The minced tissue for supplying cells for the growth of rickettsiae was then spread upon the surface. This medium, for a brief period, was undoubtedly the best means of securing the abundance of rickettsiae necessary for vaccine production.[4] Dr. Zinsser was enthusiastic over the results of laboratory tests of the method and its possibilities for mass protection against typhus. It is not known what the results were of vaccine prepared from such tissue cultures which was tested in China, Mexico and South America, nor do we yet know the full potentialities of the technic. Although Zia, a pupil of Dr. Zinsser's and working in his laboratory, had earlier shown that rickettsiae could be propagated in the chorio-allantoic membrane of the chick embryo, it was not until 1939 that this technic as modified by Cox became generally employed for the cultivation of these organisms. The typhus vaccines in use in the United States today are prepared by methods worked out in the National Institute of Health following the discovery by Topping and Shear that an important "soluble antigen" hitherto discarded, is an essential component for immunization.

In 1940 Zinsser, Plotz and Enders recommended a combination of the agar tissue culture procedure and yolk sac cultures,

[4] According to recent findings of Dr. Harry Plotz, it still affords the best method for obtaining large quantities of the rickettsia of Rocky Mountain spotted fever, surpassing the productivity of the embryonated egg.

the latter used as the source of inoculum sufficient to inoculate large areas of the agar medium spread with minced normal chick embryo tissue.

Out of Zinsser's laboratory came the useful staining methods of Castaneda and Machiavello for rickettsiae in smears and tissue cultures. A very important study of the perplexing Weil-Felix reaction (the agglutination of certain strains of bacillus proteins by the serum of typhus patients) was made by Castaneda and Zia who showed that the rickettsia of murine typhus and *B. proteus* X possessed in common an antigenic soluble factor, a "residue antigen." Undoubtedly, Zinsser gave counsel in the progress of this research.[5]

Dr. Zinsser's Writings on Education

Dr. Zinsser was rated highly as a speaker on academic occasions. Usually the theme of his addresses was education, general or medical. He also wrote a small number of essays on education for "School and Society" and other publications connected with secondary schools and universities. His intimate knowledge of three American universities, his familiarity with European universities and his personal educational experiences qualified him beyond his reputation as an educator. Nearly always in his addresses and essays on education, he took occasion to champion some one thing to be remedied or an attainable goal in education. He believed that the fate of democracy depends upon our educational system. "Education has the bear of democracy by the tail and there is no letting go." Thus we find him stressing the importance of securing, through recognition and appropriate compensation, the ablest of teachers for our high schools and preparatory schools, or making a plea for greater security in tenure of office, and for better salaries for university officers below top rank—for those men "who are ready to devote their lives to teaching, thought and investigation without ambition of riches, but with the same desires for freedom from

[5] Olitsky's 1941 Sigma Xi lecture at the University of Cincinnati—"Hans Zinsser and his studies on Typhus Fever" should be read by anyone interested in a more complete account of Zinsser's typhus work.

anxiety and for commensurate community position which are possessed by normal persons in other walks of life."

He discussed university administration with great frankness and on the whole stood for a democratic faculty rule. Of the administration of an educational institution he said: "For the guidance of an institution the success of which depends upon the imponderable value of spirit and intellect is not the mechanical driving of a machine made of iron and steel. It is more like the riding of a thoroughbred, an organism with a nervous system and a temperament. Anyone with strong legs can stay on but the riding is in the lightness of the hand and the feeling of being one with the horse. We must find our way back, or part way back, to the older conception of academic leadership, avoiding the estrangement between faculty and management which these days of reorganization have threatened to establish." The date of this quotation is 1931. Dr. Zinsser did not neglect repeatedly to point out potentialities for good and for evil in the acceptance of Foundation funds. His views on medical education concerning the correlation in teaching of the fundamental medical sciences and clinical subjects were largely the result of study and teaching in France and impressed most of his colleagues as sound and desirable to put into operation. He found opportunities to strike hard at conditions and practices of which he disapproved in scientific medical fields, among which was the "practice of patenting discoveries which directly or indirectly bear upon prevention and treatment of disease."

The most interesting theme, which recurs in varying lengths in his addresses, is that of the place of science in liberal education. Dr. Zinsser expressed his strong conviction that instruction in science should take front rank with instruction in the "humanities" or rather, that there should be no distinction for he said: "Indeed, if one could revert to the conception of the 'humanities' held by the early humanists, a modern definition of this term would include the non-specialistic and non-vocational parts of both the sciences and the liberal arts." Dr. Zinsser's conception of an educated man was one who could "properly appraise the civilization of our time." Only by reading a fair number of

his publications on educational subjects and essays for lay readers can an appraisal be made of the range and penetration of his thinking and of his activities which entitle him to rank as a "humanist."

Dr. Zinsser as Author and Poet

In the world of letters Zinsser is distinguished for his two books written for lay readers, "Rats, Lice and History" and "As I Remember Him. The Biography of R. S." Both have received great commendation from literary critics and established him in the public's mind as a writer of ability and charm who could compel sustained interest through the blending of entertainment and instruction. No one who has read these books carefully can have doubts concerning the intellectual breadth and the cosmopolitan qualities of the author's thoughts. In both books he reveals in fragmentary fashion his social, political and religious philosophies and exhibits his keen sense of humor and his mastery of ridicule and satire—always deservedly administered—as can be done only by a well stocked mind, intent upon service.

For many years Zinsser had signed his poems with the initials "R.S." for the purpose of concealing his authorship of verse from his professional brethren. His choice of these initials has been the subject of a number of speculations. Dr. John Enders, his colleague and close friend for many years, has disclosed that Dr. Zinsser told him that they stood for the initials of Rudolf Schmidt, the author of a book in German entitled "Pain, Its Causation and Diagnostic Significance in Internal Disease" which he translated into English in 1908.

"As I Remember Him" is actually an intimate and frank autobiography. Written in the third person it is more analytical and critical of its subject than any other biographer observing the rule "De mortuis nil nisi bonum" would dare or indeed care to be, because intimate knowledge of Dr. Zinsser's personality, whether revealed by actual contact or by study of his writings, cannot fail to inspire affection and respect for him. In this book Dr. Zinsser the philosopher is articulate; in it he belies to considerable degree the agnosticism he professed.

After Dr. Zinsser's death a volume of his poems was published (Alfred A. Knopf, New York, 1942) entitled "Spring, Summer and Autumn." This volume should be consulted by anyone interested in Dr. Zinsser's life. The level of merit of his verses has been highly appraised by qualified critics. His last sonnet, written shortly before his death, is in all ways his best and unquestionably will live for all time because of its beauty and popular appeal.

Hans Zinsser, the Man

In "As I Remember Him," Dr. Zinsser has revealed with little reserve his emotional and intellectual make-up. To all his revealed characteristics there should be added magnanimity in judgment of others, compassion, a great sense of justice, a rigid code of honor and great capacity for friendship.

He had great physical vigor and kept himself lean and muscular, fit for strenuous exercise and long hours in the laboratory. That he was able to accomplish so much in diverse fields was because he commanded, jealously and firmly, the disposal of his time. He served on an astonishingly large number of committees and boards. He was a Trustee of the Massachusetts General Hospital. He was on the editorial boards of five scientific and medical journals. He was a member of more than a dozen fraternities, clubs and honorary societies, and of 36 scientific societies, including the Association of American Physicians, American Philosophical Society, National Academy of Sciences, American Academy of Arts and Sciences, National Academy of Tropical Medicine, American Association for the Advancement of Science, New York Pathological Society (President, 1915), American Association of Immunologists (President, 1919) and Society of American Bacteriologists (President, 1926).

Many honors came to Dr. Zinsser. He received honorary Doctor of Science degrees from Columbia University, 1929, Western Reserve University, 1931, Lehigh University, 1933, Yale University, 1939 and Harvard University, 1939. His decorations were: Distinguished Service Medal, U. S. A., Order of Saint Sava, Serbia, and Legion d'Honneur, France. The Sedgewick Memorial Medal of the American Public Health

Association was awarded him posthumously in October, 1940.

At the time of his death, shortly before his sixty-second birthday, Dr. Zinsser, in addition to his Professorship of Bacteriology and Immunology, held the positions of Chief of the Bacteriological Services of the Children's and Infants' Hospitals of Boston, and Consultant in Bacteriology at the Peter Bent Brigham Hospital.

He is survived by his widow, a son and a daughter. The son, Hans Zinsser, is a graduate of Harvard College, 1938, and of the College of Physicians and Surgeons, Columbia University, 1942, and saw service in Europe as a surgeon with paratroopers. In 1940 he married Anne Drinker, daughter of Professor Cecil Kent Drinker. Dr. Zinsser's daughter is the wife of Vernon Monroe, Jr. of New York City.

Although the writer enjoyed the friendship of Dr. Zinsser for many years and was honored by his confidences during the last few years of his life, and in spite of the perspective afforded by the lapse of six years since his death, he finds it impossible, because of his own limitations, to portray him adequately. He had some attributes of the animal he loved most—the thoroughbred horse—sensitive and spirited, intolerant of restraint, with barriers of reserve penetrable only by those to whom confidence and affection are given.

The aggregate of his scientific work in bacteriology and immunology is impressive and although he made no single great advance, many of his contributions are now important elements in the structure of immunology. His textbooks are outstanding because of his great knowledge and integrative ability. He was an able and much loved teacher of undergraduates. To graduate students working in his department (and they came from many countries) and to members of his department, he was more than a leader in common scientific interests. He was an awakener of new intellectual interests in other fields. I quote now from Dr. John F. Enders' memorial address—"But the extent of his general knowledge was sufficient to furnish forth

respectably two or three professor's chairs. The world of letters only recently has become aware of his broad cultivation, but for us it was an old and delightful story. For some years we all lunched together in the laboratory. As we ate, the conversation—led by him—became animated. Literature, politics, history and science—all he discussed with spontaneity and without self-consciousness. Everything was illuminated by an apt allusion drawn from the most diverse sources. . . . Here, indeed, was a liberal education to be gained pleasantly while one dined. Under such influences, the laboratory became much more than just a place to work and teach; it became a way of life."

From many sources we learn that Dr. Zinsser was deeply interested in people as persons. He was generous with his time and money in cases of need. He would go to great trouble to set matters right if he learned that he had been unjust to another person by act or thought. In his travels he sought close contacts with substantial representatives of all walks of life—fishermen, laborers, peasants, artisans, professors. Intensely patriotic and a firm believer in democracy, his culture was cosmopolitan—continental and patterned unconsciously after French standards. He felt at home in France. The way of living there suited him and temperamentally and intellectually he blended perfectly in French university circles. The late Lawrence J. Henderson is quoted as having said: "Although Hans Zinsser is almost wholly of German descent, he is the most French man whom I have ever met." Undoubtedly this was spoken in great approbation since Whitehead has said that ". . . the mentality of men like Jefferson and Franklin was French. There, indeed, was the homeland of their thoughts."

Dr. Zinsser lived a few months over two years after knowing that he had a fatal disease—leukemia. During this period he worked harder than ever and outwardly at least was composed and lived the life of a man thoroughly engrossed in his research work and keenly interested in everyday affairs and in his friends.

While observing the progress of his disease and awaiting death he displayed those qualities of spirit and courage about which he wrote in 1927 in admiration and affection upon the death of

Dr. Francis W. Peabody: "Indeed the proof of a man's life—how much has been the living of a formula and how much an inward light—may often be found in the manner of his facing death. For courage is still, as it has always been, a thing of great beauty that springs, whatever its form of expression, from an inner source of moral power. We wish for ourselves and the ordinary human being, a swift and merciful death, which is most easily supported with dignity and composure. For him we would not have had it other than it came. Those who were fortunate in seeing him during those eighteen months when he and death sat face to face—who dreaded their first visits and came out glad and inspired with a new faith in the nobility and courage to which rare men can attain—these knew that the ugliness and cruelty of death were defeated."

Dr. Zinsser acquired an extraordinary number of admiring friends scattered over the world because he was a man of great charm and many talents and because his reactions in all situations were impulsively honest and understandable. To sincere persons he freely revealed his real self; to make use of artificiality in manner, pose or dress for the purpose of creating an atmosphere of importance was not in his nature. Throughout his life he carried the aura of youth and all who became cognizant of his many interests and enthusiasms, hopes and aspirations acquired great affection for him.

The writer is indebted to Mrs. Zinsser for several letters containing much information about the early years of Dr. Zinsser.

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KEY TO ABBREVIATIONS IN BIBLIOGRAPHY

Am. J. Digest. Dis. and Nutrition = American Journal of Digestive Diseases and Nutrition
Am. J. Hyg. = American Journal of Hygiene
Am. J. Pub. Health = American Journal of Public Health
Am. Med. = American Medicine
Arch. de l'Inst. Past. de Tunis = Archives de l'Institute Pasteur de Tunis
Arch. Dermat. and Syph. = Archives of Dermatology and Syphilology
Arch. Int. Med. = Archives of Internal Medicine
Arch. Path. = Archives of Pathology
Boston Med. and Surg. J. = Boston Medical and Surgical Journal
Bull. N. Y. Acad. Med. = Bulletin of the New York Academy of Medicine
C. R. de la Soc. de Biol. = Comptes Rendus des Seances de la Societe de Biologie
Harvard Alumni Bull. = Harvard Alumni Bulletin
Harvard Grad. Mag. = Harvard Graduate Magazine
Harvard Univ. Press = Harvard University Press
J. A. M. A. = Journal of the American Medical Association
J. Bact. = Journal of Bacteriology
J. Exp. Med. = Journal of Experimental Medicine
J. Immunol. = Journal of Immunology
J. Indust. Hyg. = Journal of Industrial Hygiene
J. Lab. and Clin. Med. = Journal of Laboratory and Clinical Medicine
J. Med. Res. = Journal of Medical Research
J. Preven. Med. = Journal of Preventive Medicine
J. Roy. Army Med. Corps = Journal of the Royal Army Medical Corps
Med. Rec. = Medical Record
Mem. Nat. Acad. Sci. = Memoirs, National Academy of Sciences
Mil. Surgeon = Military Surgeon
Milton Grad. Bull. = Milton Graduate Bulletin
Nat. Med. J. China = National Medical Journal of China
N. E. J. Med. = New England Journal of Medicine
N. E. Quarterly = New England Quarterly
N. Y. Med. J. = New York Medical Journal
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Proc. N. Y. Path. Soc. = Proceedings of the New York Pathological Society
Proc. Path. Soc. Phila. = Proceedings of the Pathological Society of Philadelphia
Proc. Soc. Exp. Biol. and Med. = Proceedings of the Society for Experimental Biology and Medicine
Trans. Am. Neisserian Soc. = Transactions of the American Neisserian Society
Trans. Assoc. Am. Phys. = Transactions of the Association of American Physicians

Trans. Coll. Phys. = Transactions of the College of Physicians
Trans. Coll. Phys. Phila. = Transactions of the College of Physicians of Philadelphia
War Medicine, Paris = American Red Cross Society in France

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For the preparation of this list of publications I am indebted to Miss Rosanna D. Thorndike, Professor Zinsser's secretary from 1923 to 1940.

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NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XXIV—ELEVENTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

THOMAS MIDGLEY, JR.

1889–1944

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CHARLES F. KETTERING

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1947

Midgley, Sr., was a prolific inventor in a variety of fields, but notably in that of automobile tires. And his mother, Hattie (Emerson) Midgley, was the daughter of James Emerson, inventor of the inserted-tooth saw. One of Midgley's earlier ancestors is believed to have been a trusted employe of the great James Watt. When young Midgley was about four, his family moved with him to Trenton, New Jersey, and two years afterwards to Columbus, Ohio. In Columbus, Midgley lived so many years at different times in his life that that city was always home to him, and he was living in the country near Columbus at the time of his death.

Midgley attended the public schools in Columbus until partly through high school. Always interested in athletics, he played on his high school baseball and football teams. When the spit ball first began to be used by baseball pitchers, Midgley and Sandford Brown, one of his teammates at the time and a lifelong friend, made a search for a substance to give the ball enough slippiness to make it have the maximum curving effect. Midgley hit upon the idea of using an extract of the inner bark of the slippery elm as an aid to that end, a practice which was later followed extensively by baseball pitchers.

Then in 1905 Midgley went to Betts Academy at Stamford, Connecticut, to prepare for college. At Betts Academy also he played both baseball and football, and in his studies he showed that he had an original turn of mind. In geometry, for example, he delighted to solve a problem by a method different from that illustrated in the textbook or from that demonstrated by the teacher. It was from his teacher of chemistry at Betts Academy, Professor H. M. Robert, that Midgley gained an interest in the periodic table which continued throughout his life and which later helped guide him to two of his most important discoveries. During the years of his chemical researches Midgley's interest in the periodic table caused him to carry a copy of it with him constantly. In his Perkin Medal Address in 1937—an address which he

entitled "From the Periodic Table to Production"—Midgley told of the important place the periodic table had had in his chemical endeavors (46).* He said then that it had served as his guide in the latter and successful phase of the "fox hunt" which led to the discovery of tetraethyl lead as an anti-knock agent, and also that in the search for a nontoxic and noninflammable refrigerant it had pointed the way for him to make use of the unpromising but effective element, fluorine.

From Betts Academy, Midgley entered Cornell University, where he took the course in mechanical engineering. Dean Dexter S. Kimball recalls that during Midgley's years at Cornell he showed the great curiosity of mind which was so characteristic of his life. His interest in experimentation was so great that he did not have time for many of the usual student activities, although he did at one time organize an aviation club among the students. The club had no airplane, not even a glider, but it was perhaps one of the first such organizations to be formed. Midgley's absorption in making side excursions into other fields was such that it often interfered also with his attention to the routine requirements leading to an engineering degree. He was nevertheless a good student, and he was also one whose friendly ways made him many friends.

After graduating from Cornell in 1911, Midgley went to work for the National Cash Register Company at Dayton, Ohio. There he was a draftsman and designer in Inventions Department No. 3. That department, which was then in charge of W. A. Chryst, was the same one in which I myself had begun work after leaving college seven years earlier, and in which the several improvements in the cash register which I had made were developed.

After working a year at the National Cash Register Company, Midgley left at the request of his father to assist him in an effort to improve cord tires and tread design. He thus

* Figures in parentheses are the numbers of the titles in the appended bibliography.

became before long chief engineer and later superintendent of the company formed to manufacture the improved tire. During that period Midgley's interest in the automobile and his experimental bent caused him to conceive a simple form of hydrometer for indicating whether automobile cooling systems contained enough alcohol in the winter time. His hydrometer was an unconventional one in that it consisted of two rubber balls of such difference in specific gravity that when put into a radiator fluid which contained the right amount of alcohol one of the balls would float and the other would sink. But, as the tire venture did not prove successful, the factory soon had to be closed, thus ending the hydrometer endeavor also and making it necessary for Midgley to look for other employment.

During his year at the National Cash Register Company, he had learned about the research which had been conducted under my guidance while I was head of Inventions Department No. 3. That recollection, together with the belief he had meanwhile come to have that research was the work he himself was cut out to do, caused him to make a decision which years later he said was the most important one of his whole life. He decided to try to get a job with me in the organization I had meanwhile developed, the Dayton Engineering Laboratories Company. So, as W. A. Chryst had meanwhile left the National Cash Register Company and had become chief engineer of this new company of ours, Midgley went to Dayton and asked him for an opportunity to help out in our research endeavors. It was thus that in 1916 Midgley became a member of our research staff and began then his long association with me and his remarkably productive career in research.

At that time Midgley was twenty-seven years of age, and his most highly productive work was done in the fifteen years following that time. Consideration of this circumstance may have caused Midgley to present as his presidential address before the American Chemical Society in 1944 a paper

entitled "Accent on Youth" (56). In that address he included an analysis of a large group of outstanding inventions from the records of the United States Patent Office, which showed that 90 per cent of them had been made by men under 45. "It would seem foolish," he commented then, "to increase the time required for formal education beyond what it now is, since such increases would definitely encroach upon the most valuable years for actively prosecuting research and development." He said, though, that "being 27 and in uniform does not generate the genius of Napoleon."

Midgley's first job with us was to finish up a project already begun, the development of a built-in hydrometer for indicating the degree of charge in the storage battery of a Delco-Light farm lighting set. That job was quickly done and Midgley then asked, "What do you want me to do next, Boss?" That simple question and the answer to it turned out to be the beginning of a great adventure in the life of a most versatile man. So great did Midgley's love for research prove to be that shortly before his death he said that for him those next few years after he asked the question were like a story from the Arabian Nights.

Ever since we had put battery ignition and the self-starter on cars, the noisy bugbear of knock in automobile engines, which about that time had begun to be pronounced, had been blamed on the battery-type ignition. Some investigation of the subject had been made, but the starter and ignition business had grown so rapidly that for simple lack of time and hands the instruments and data were put away in a closet for future reference. When Midgley asked me that question, we sat down and talked about the knock in engines and why an exact knowledge of the cause was important. And I suggested that he get out those instruments, chief of which was an old Dobbie-McInnes manograph, put it on a Delco-Light engine and see what he could find out.

In that first investigation Midgley showed his most important characteristics as a research man—versatility and action.

The old manograph was not good enough to do just what was required. It did show, however, that the knock did not come from preignition, as was the common belief, but that it was caused by an abrupt rise in pressure *after* ignition by the spark plug had occurred. So Midgley said, "Let's make a better indicator to study it." And he proceeded to do that. Never did he say "This doesn't work," and stop there. Always he would say instead, "How can we overcome the difficulty and move ahead?"

The new knowledge gained in this work brought up the question, why does the abnormal rise in pressure after ignition occur and how can it be stopped? In talking over the problem we thought that maybe if the fuel were colored red it would absorb more radiant heat and evaporate more completely, thus preventing the rough combustion. This theory came to us then because we both happened to know that the leaves of the trailing arbutus are red on the back and that they grow and bloom under the snow.

In searching for a dye to color the fuel red and so to test our surmise, Midgley found no oil-soluble dye available there at the time, which happened to be on a Saturday afternoon. So he accepted iodine from the chemical store as a substitute. And, much to our astonishment, dyeing the fuel red with iodine did stop the knock completely. But was it the color or some other property of iodine? Red aniline dyes were soon obtained and tried with completely negative results. But colorless ethyl iodide did stop the knock, just as iodine had done. So evidently it was the iodine itself and not the color.

Of this new field Midgley then said, "No one knows anything about this problem. We must outline a whole series of experiments and find out if it is physics or chemistry." Unknowingly he began with that outline to become himself both a physicist and a chemist, but unhampered by the traditions of either. Our laboratory then was on the second floor of an old tobacco warehouse and not very well equipped, but that made no difference to Midgley.

By that time World War I was upon us, and Midgley then began to devote his efforts, along with men from the United States Bureau of Mines, to finding a better aviation fuel, one capable of giving higher engine outputs. The knock was limiting the power of the new Liberty engine. It was reported that the Germans had been using cyclohexane as fuel in their fighter planes. And, although the report was not true apparently, the work which Midgley and his group had done nevertheless made it appear that cyclohexane should make it possible to use considerably higher engine compressions, and thus give more power than the aviation gasoline of that time. So now Midgley turned chemist and tried to make cyclohexane by hydrogenating benzene which was readily available in quantity, but in doing so he experienced all the difficulties and disappointments of which he had been warned. However, Midgley was a tireless worker and long hours were common. So, after some months spent in overcoming obstacles, the most troublesome of which was the destructive effect of sulfur on the catalyst, the problems of hydrogenating benzene in quantity were solved.

Many barrels of this new fuel, which consisted of 70 per cent cyclohexane and 30 per cent benzene, were manufactured and tested successfully, both on the ground and in the air. That fuel was perhaps the first synthetic aviation gasoline. The end of the war came before the new fuel got into service, but out of that work and the investigation of other compounds which had preceded it came an important realization—the realization that it is the molecular structure of a fuel which controls freedom from knock, and not such physical characteristics as density and volatility, as had been supposed.

During World War I our laboratory was given also the job of developing an aerial torpedo similar to the buzz bomb of World War II. Midgley was assigned the additional responsibility of developing the control systems for that device. And the project, which proved quite successful, but which was not

used in combat either because the time was too short, benefited greatly from Midgley's contributions to it.

In that war-time effort to hydrogenate benzene Midgley met with an accident which brought out an early instance of his originality and ingenuity. The fusible plug in a hydrogen tank blew out and spattered particles of the soft metal into the cornea of one of Midgley's eyes. The doctor, finding that the particles were located in places too delicate to permit being picked out, tried treatments to soften the cornea, but with little benefit. After some days of discomfort, Midgley himself conceived and tried—with the consent of his doctor—removing the particles by bathing his eye frequently with purified mercury. That experiment proved effective, and the eye was soon restored to normal. The ingeniousness of the treatment so impressed his friend and boyhood chum, E. J. Crane, now editor of Chemical Abstracts, that he wrote an account of it for Industrial and Engineering Chemistry.* And that account perhaps contains the first mention of Midgley's name in the publications of the American Chemical Society, where it appeared so frequently and with such high distinction in later years.

After the war we again took up in a serious way the search for a practical antiknock agent. Although neither the discovery of iodine as a knock suppressor nor the synthesis of cyclohexane for airplane fuel were put to practical use, they did have this important effect. They changed Midgley's principal interest and activity from the field of engineering to that of chemistry, as seeming to him by that time to be more interesting and important. And thus, being a very versatile person, he at length became one of the best informed, and surely one of the most creative chemists in the world.

The search for a practical antiknock agent was actively pursued for three years more, with the customary difficulties and disappointments of such endeavors, before the discovery

* Industrial and Engineering Chemistry, *11*, 892 (September, 1919).

of tetraethyl lead. Many antiknock agents were discovered along the way—compounds of iodine, of nitrogen, of phosphorus, of arsenic, of antimony, of selenium, of tellurium—but every one had some limitation or shortcoming which prevented it from being used in a practical way. Even after the discovery of tetraethyl lead, the research was continued for about three years longer before the several problems of using lead in an engine had been solved in a satisfactory manner. Bromine was found necessary to the solution of one of those problems—that of preventing the formation of lead oxide during combustion and its deposition on valves and in the combustion space. Since it appeared that for the purpose bromine would be needed in amounts much larger than had ever been available before, an intensive search was made for further supplies of it. That hunt led to the research which demonstrated that bromine could be extracted from the sea, where it is present in inexhaustible amount, but in concentration extremely minute—a concentration of only 65 parts per million.

After the discovery of tetraethyl lead and the development of a satisfactory antiknock compound containing it, Midgley went to work on the job of introducing the new product to the public, an endeavor in which he met with, and finally overcame, many obstacles and much opposition. It was thus in his years of work on the antiknock problem that Midgley demonstrated unusual talents in all three of the important phases of industrial research: first, in original investigation or invention; second, in development or in conversion to the stage of practical usefulness; and, third, in selling the new thing to the public—or in some instances to management first.

Something of Midgley's showmanship or ability in salesmanship was seen by those who heard him, and saw him, give his scientific papers. In presenting the original paper on the discovery of the antiknock compounds, for instance, he made extensive and striking demonstrations of knock, both in a glass tube and in an engine, showing how knock could either

be made worse or eliminated altogether by chemical means (4). So also, in presenting the first account of the research which yielded the fluorine-containing refrigerants, he demonstrated both their nontoxic and their noninflammable properties in one breath, so to speak. This he did by taking his lungs full of the vapor of one of the compounds and then softly exhaling it to surround and extinguish a candle burning before him (26).

After the sale of gasoline containing tetraethyl lead had been successfully established, Midgley went back to the research laboratory. The refrigeration industry was then in bad need of a new and better refrigerant, particularly one for use in air conditioning which would not take fire and which would be free of harmful effects upon people who might be exposed to it. Again with the periodic table as a guide, Midgley came to the conclusion that any *new* compound which could have suitable physical properties, would have to contain fluorine. And so, in spite of warnings about the hazardous nature of fluorine and of such misgivings as he himself may have had, he and his helpers prepared such a compound, dichlorodifluoromethane. This is the compound which today is commonly known as Freon, and it proved to have just the properties required. It is highly stable, noninflammable, and altogether without harmful effects on man or animals.

But he almost missed that important discovery; for, of the three or four small batches of the new compound made with the total available raw material of the time, only the first was not toxic to the animals on which it was tested. The others behaved just as everyone expected fluorine compounds to behave, that is, the animals exposed to their vapor quickly died. But that, as it was found later, was due not to the Freon itself but to an unsuspected impurity in the compound. Fortunately, though, it was the first batch which turned out well. In the toxicity tests made immediately on that first sample it proved so completely free of harmful effects on the animals used in the trial that in the final phase of the test all the nitrogen in the air the animals breathed was replaced

with Freon vapor without harm to them. The fluorine-containing refrigerants, of which there are a number with different vapor pressures, have since been used extensively in the refrigeration and air-conditioning industry, and have completely filled the need for which they were sought. Also, the high volatility and completely nontoxic character of Freon made it the ideal substance to fill an unforeseen need. That was as a means of dispersing insect repellents in confined spaces, and during World War II it was put to extensive use for that purpose.

Midgley's extensive researches on rubber were undertaken because of a life-long interest in the subject, although that interest was heightened by high prices and a threatened shortage of rubber at the time. With the help of a few associates, he made extensive studies of the composition of natural and synthetic rubbers and of the chemistry of vulcanization. This work resulted in the publication of a series of nineteen outstanding papers. While nothing of a commercial character came of those researches, Midgley considered the work he did on rubber as the most scientific of all his endeavors. And among those informed in the field he received a great deal of recognition for it.

In the matter of recognition for his scientific endeavors, Midgley was particularly fortunate. He received all four of the most important medals for chemical achievement: the Nichols Medal of the New York Section, American Chemical Society, 1922; the Perkin Medal of the Society of Chemical Industry, 1937; the Priestley Medal of the American Chemical Society, 1941; and the Willard Gibbs Medal of the Chicago Section, American Chemical Society, 1942. For his pioneering work on engine indicators, The Franklin Institute awarded him the Longstreth Medal in 1925. He was elected to membership in the National Academy of Sciences in 1942. He also received the honorary degree of Doctor of Science from the College of Wooster in 1936 and from The Ohio State University in 1944. The citation read by Prof. William

Lloyd Evans at the time Midgley received the latter degree was in part as follows:

> The research work of Mr. Midgley has received wide recognition, as is evidenced by the great number of distinctions which have come to him from those groups best qualified to evaluate his contributions to human knowledge. Through experience, the layman will also testify his indebtedness to one who has contributed so greatly to more pleasant and efficient living. He has made science a liberator, and we rejoice with him in the satisfactions that must be his in seeing the fruits of his labor. Posterity will acknowledge their permanent value.

Midgley had a large part in the business side of the industries which came out of his endeavors. He was vice president of the Ethyl Corporation from the time of its formation, as well as the first general manager of that company. As such he contributed a great deal to the success of the enterprise, both in solving commercial problems and in overcoming the prejudice against the new product which arose from fear that the use of lead in gasoline would poison people. He was vice president of Kinetic Chemicals, Inc., (Freon) and a director of the Ethyl-Dow Chemical Company (bromine from the sea). One of the satisfactions he had was that so many thousands of workers found employment in the new enterprises which grew out of the research endeavors in which he had such a dominant part.

Midgley was a strong believer in research and he did everything he could to advance its application. As a pioneer in research on fuels, he had a considerable part in the founding of some of the most important and productive research laboratories in the petroleum industry. He was for some years a director and vice president of The Ohio State University Research Foundation. In a paper entitled "The Future of Industrial Research," presented less than a month before his death, he said, "I am of the opinion that, as time goes on, more and more research of the fundamental type will be necessary" (57). As a means of helping to insure that there will

be trained men to conduct such research, he advocated that "by ample fellowships both in size and number, it (industry) should encourage many young men to remain in educational work."

Midgley believed also in the importance and the soundness of the United States patent system. "I believe it is the purpose of our patent system," he said, "to stimulate competitive research on applied subjects. . . . To my way of thinking, . . . any increase of control over our environment, or beneficial alteration thereof, is invention. The method by which such results are obtained is of no importance." He was chairman of the executive committee and an active organizer of the Centennial Celebration of the American Patent System in 1936, and he performed the same function also in the celebration of the United States Patent Law Sesquicentennial in 1940.

Having been a many-sided person, Midgley did other things than those discussed above, of course, and a few instances of such further activity may be cited. He discovered one of the first known catalysts for cracking hydrocarbons, iron selenide, a catalyst which produced compounds of the aromatic type (15). In addition to his development of the Midgley optical gas engine indicator mentioned earlier (1), he directed the later development of the widely used bouncing-pin indicator (5). He was a pioneer in the investigation of engine combustion, first by visual observation of the flame through a window in the chamber, then by spectroscopic means (6), and finally by measurement of the amount of radiant energy emitted (16).

Midgley was a firm believer in the worth of scientific societies. He was a member of several such societies, including the National Academy of Sciences, the American Association for the Advancement of Science, the American Chemical Society, the American Institute of Chemical Engineers, the Society of Automotive Engineers, and the American Society for Testing Materials. He was a member also of Alpha Chi Sigma, Phi Kappa Phi, Tau Beta Pi, the Society of Sigma Xi, the

Chemists' Club of New York, and the Dayton Engineers' Club. He was an influential delegate to the International Congress on Chemistry, held in Rome in 1938.

Midgley's extensive service to chemistry through the American Chemical Society as a Director for 14 years, as chairman of the Board of Directors for 10 years, and as President in 1944, has already been related. In respect to that long and unselfish service of his, this was said in a resolution of his fellow Directors of the Society after his death:

His sound judgment, high ideals, alert energy, and kindly human contacts brought success to the American Chemical Society, raising the standards of the whole chemical profession and endearing him to his fellow Directors and to all who served with him. In the truest sense his life represented the best thought, far-reaching vision, and the most practical accomplishments in the field of chemistry of his day and generation.

Midgley had, too, many interests outside the fields of his researches. He was a careful student of history and an investigator of many things in nature, one of which was the structure of ant hills. During the period of Midgley's active research, he took up golf as a means of getting out of the laboratory at intervals. Never having played the game before, he studied books on the subject and he talked to professionals on the mechanism of the swing. The result was that in a short time his golf score was down in the low 70's. Later, his observations of the deficiencies of greens set him to experimenting with grasses on his estate near Columbus. And soon greens experts from all over that area were coming there to see what he was doing, although by that time he himself had quit playing golf.

Midgley had a great fondness for music. He assisted some promising musicians to further their training. And once, when he came to know a young man who was working on an improved recording mechanism, he offered him his support, both technical and financial. The result was that, besides aiding in the project, he accumulated an extensive library of

recordings, some of sports events, and some made from the wings of the Metropolitan Opera.

Midgley had also a liking for poetry and an aptitude for producing it himself. Some of his poetry was included in his Presidential Address to the American Chemical Society, "Accent on Youth" (56), and it ended with these two lines, lines which might well apply to Midgley himself:

Let this epitaph be graven on my tomb in simple style,
"This one did a lot of living in a mighty little while."

In the early fall of 1940 Midgley was struck by an acute attack of poliomyelitis, which, in spite of the care taken during his illness and of all the efforts made afterwards by himself and others, deprived him of the use of his legs and made him a semi-invalid. In typical spirit, Midgley computed afterwards the statistical probabilities of a man of his age catching polio, and his answer came out, as he expressed it, "substantially equal to the chances of drawing a certain individual card from a stack of playing cards as high as the Empire State Building." Nevertheless, he said, "It was my tough luck to draw it." But with characteristic courage and energy he continued in many of his activities in spite of the handicap right up until the time of his death, notably in his service to the American Chemical Society. He served also during World War II as vice chairman of the National Inventors Council and as head of one branch of chemical endeavor for the National Defense Research Committee.

On August 3, 1911, Midgley married Miss Carrie M. Reynolds of Delaware, Ohio. Two children were born to them, Jane (Mrs. Edward Z. Lewis), and Thomas Midgley, 3rd. All of Midgley's immediate family except his father survive him.

Midgley liked people of every class and profession. As he himself put it once: "I have always had a fondness for intelligent people." He had the ability to mingle with and to enjoy the company of people from all walks of life, and he would be as much interested in the philosophy of a cab driver

as in the opinions of men in higher walks of life. Out of his manifold activities and associations, he made many friends, and he liked nothing better than being a host to them. To me personally Midgley was, throughout all those years, like a son or a brother. And he was held in the highest esteem by his associates everywhere.

Midgley died unexpectedly on November 2, 1944, at the age of 55. At his funeral, the minister read the familiar verse, "We brought nothing into this world, and it is certain we can carry nothing out." It struck me then that in Midgley's case it would have seemed so appropriate to have added this: "but we can leave a lot behind for the good of the world." And what Midgley did leave behind is a great heritage to the world from a busy, a diversified, and a highly creative life.

KEY TO ABBREVIATIONS USED IN THE BIBLIOGRAPHY FOLLOWING

A.P.I. Bul. = Bulletin of the American Petroleum Institute.
Chem. and Eng. News = Chemical and Engineering News. (Formerly News Edition of Industrial and Engineering Chemistry).
Ind. Eng. Chem. = Industrial and Engineering Chemistry. (Formerly Journal of Industrial and Engineering Chemistry.)
Ind. Eng. Chem., Anal. Ed. = Industrial and Engineering Chemistry, Analytical Edition.
J. Am. Chem. Soc. = Journal of the American Chemical Society.
J. Phys. Chem. = The Journal of Physical Chemistry.
Motor = Motor (New York).
S.A.E. Jour. = S.A.E. Journal. (Formerly Journal of the Society of Automotive Engineers.)

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